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BULLETIN OF THE HISTORY OF MEDICINE

UNIVERSITY OF MICHIGAN
MAY 23 1955
MEDICAL

ORGAN OF
THE AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE
AND OF
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

VOLUME XXIX MARCH–APRIL, 1955 NUMBER 2

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THE JOHNS HOPKINS PRESS
BALTIMORE 18, MARYLAND

BULLETIN OF THE HISTORY OF MEDICINE

ORGAN OF
THE AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE
AND OF
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

Founded by HENRY E. SIGERIST

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Manuscripts, review copies of books, and correspondence relating to the publication of papers should be addressed to The Editor, Bulletin of the History of Medicine, 1900 E. Monument St., Baltimore 5, Md.

Contributors of articles will receive 25 reprints without covers free of cost. An order form for additional reprints, giving the cost, is sent with the galley proof.

The Bulletin is published bimonthly. One volume of at least 500 pages is published during the year. The subscription rate is $6.00 a year in the United States and countries of the Pan-American Union, $6.25 in Canada, and $6.50 in other foreign countries. Single numbers when available are $1.50 each. Prices for back volumes may be had on application. Volume I is out of print. New subscriptions are entered to begin with the first issue of the current volume.

Active and Non-resident Members of the American Association of the History of Medicine receive the Bulletin on payment of $6.00 annual dues, of which $5.00 is for a year's subscription. Constituent Societies pay $10.00 annual dues, of which $6.00 is for a year's subscription to the Bulletin.

Claims for replacement of missing numbers must be received within 3 months (6 months in the case of foreign subscribers) following publication of the number.

THE JOHNS HOPKINS PRESS

BALTIMORE 18, MARYLAND, U. S. A.

PUBLISHED BIMONTHLY

January-February, March-April, May-June, July-August, September-October, November-December

Made in United States of America

Entered as second-class matter January 3, 1935, at the post office at Baltimore, Maryland, under the Act of March 3, 1879.

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CADWALLADER COLDEN AS A STUDENT OF INFECTIOUS DISEASE

SAUL JARCHO

Dr. Cadwallader Colden (1688-1776), Lieutenant Governor of the Province of New York, combined within the span of a long lifetime many interests and many activities which make him an appropriate subject for consideration by historians of medicine. Since there seems to be less time in the twentieth century than there was in the eighteenth, I have selected for discussion only those parts of Colden's vast work which deal with the infectious diseases.

A few biographical details will be necessary as background (1-8). Colden was born in Ireland, to Scottish parents, in 1688. Probably his birthplace was Enniscorthy in County Wexford. After receiving the Edinburgh baccalaureate in 1705, he studied anatomy, chemistry, and botany for three years in London. In 1710 he removed to Philadelphia, where he practised medicine and also engaged in business ventures. In 1718 he was persuaded by Governor Robert Hunter of New York to embark on a career in government. At this time he abandoned the regular practice of medicine. In 1720 he became Surveyor General of New York. In the following year he was made a member of the Governor's Council; this appointment he held for 55 years. He was Lieutenant Governor from 1761 to 1776. Throughout his career, which ended in 1776, he was a

faithful servant of the British crown. In recognition of this his fellow New Yorkers burned him in effigy on Bowling Green during the Stamp Act Riots of 1765 (9-11).

In the world of letters Colden is known best for his *History of the Five Indian Nations of Canada*, first published in 1727 (12). His *Explication of the First Causes of Action in Matter and of the Cause of Gravitation* (13) has slowly risen to a high place in the esteem of philosophers. A historian of philosophy, I. Woodbridge Riley (14), refers to Colden in the following terms: He was "the first and foremost of early American materialists . . . His system in general was in advance of the ordinary eighteenth century deism, anticipated to a degree the New England transcendentalism, and issued in a movement essentially modern,—the resolution of matter into the mechanics of energy . . ." Colden wrote also on surveying, fluxions (calculus) (15), astronomy, electricity, cancer (16, 17) and deontology. He played a part in the formation of the American Philosophical Society. He introduced the Linnaean system into America, and prepared descriptions of 237 American plants; this material was published by the illustrious Linnaeus (18). In the twelve volumes of Colden's published letters and papers (19, 20, 21) there are interchanges with Linnaeus, Gronovius, Franklin, Mitchell, Fothergill, and John and Samuel Bard. Colden befriended John Bard and interested himself in the education of young Samuel Bard, and Samuel received instruction in botany from Colden's daughter Jane (22).

For our present limited purposes we may summarize Colden's life story by observing that he was a Scottish minister's son and was educated initially for the ministry, that he studied at Edinburgh and London, that he practised medicine regularly for not more than eight years, that his duties as public official were diverse and onerous, that he travelled extensively as surveyor and as a negotiator of Indian affairs, and that much of his life was divided between the physical isolation of upstate New York and the intellectual desert of provincial politics. As a corollary and consequence, much of his writing, both on medicine and on other subjects, was speculative.

How did this eighteenth-century American materalist philosopher and physician view the infectious diseases which prevailed in the thirteen colonies in his time?

He tells us that he was "unwilling to theorize where a matter can be decided by observation" (23). To John Mitchell he wrote that "mere speculation . . . must always give place to that knowledge which arises from accurate and judicious observation . . . Your knowledge, arising

from observation, is upon a much surer foundation than mine" (24). To Gronovius he wrote that it is dangerous "to trust to experiments unless they be performed with all requesite precaution" (25). To John Bard he wrote that he had been educated in the "Mechanic Phylosophy" but "the Mechanical principles do not reach to every part of the Animal Oeconomy" (26).

What were the considerations which caused this strain on Colden's mechanical (i. e. iatromechanical) preconceptions? Part of the answer is found in a long letter from Colden to John Bard, dated July 5, 1758 (27). Colden's son David, sickly and kyphoscoliotic, had requested inoculation for smallpox. After a long discussion of the boy's physique, the risks, and the prevailing constitution of the atmosphere, Colden remarked in his letter:

> A very small bit of Thread moisten'd with the matter from the Pustule of the Small Pox, and put into a slight incision of the Arm or other part of the body produces certain and surprising changes, in the Animal Oeconomy. Can any Man account on mechanical principles, why this small portion of Matter infused into the capillary Vessells lies to all appearance at rest 6 or 7 days & afterwards produces such extraordinary appearances? . . . Many such like casses happen in the Animal oeconomy, in which Theory absolutely fails the Physician and his whole skill depends on accurate observation of the natural course & crisis of that distemper, & of what is beneficial & what injurious . . . I can in no manner account for the appearances on mechanical principles or from Physical causes, otherwise than from an anology [sic] with what is observed in fermenting Liquors, viz that the variolous pus is a kind of leven or ferment, which being mixed with the Blood of a living Man produces a fermentation or fever . . . That the whole art of cure of the desease, simply taken, consists in regulating the fermentation . . .

The perplexities described are those of the iatromechanist confronted by the phenomena of infection, in a single case and artificially induced. As we shall see, Colden experienced similar philosophic pangs while considering epidemic outbreaks of yellow fever.

Colden's letter to Bard contains at least one incidental comment (28) which deserves much more than incidental notice. He remarked that the "visions," i. e. the psychic disturbances, which occur in smallpox are proportionate to the *quantity* of variolous matter assimilated. This simple suggestion represents a distinct advance over the tradition of attributing clinical variations to vague imponderables.

Another aspect of Colden's ideas about infection can be observed in his "Account of the Climate of New York." The earliest form of this essay which I have been able to trace is an official state paper prepared by Colden in his capacity as Surveyor General and forwarded to London

by Governor Burnet in 1723 (29). A slightly edited version was published in 1811 (30).

In this treatise the Surveyor General points out that in the spring, on the breaking up of a hard winter, the people are subject to pleurisy and inflammatory fevers. "Country people and such as are most exposed to the cold are most liable to these Distempers." In July, August, and early September, the sickliest part of the year, the prevalent diseases are intermitting fevers, cholera morbus, and fluxes. Intermitting fevers are less common in this province than in those farther south, but fluxes are more common in New York City than in Philadelphia. These local differences in frequency are referable to the ingestion of fruit and to differences in the brackishness and hardness of the waters. Consumptions and diseases of the lungs are uncommon in New York because the air is clear and its elasticity is strong.

Thus pleurisy and inflammatory fevers—perhaps this is meant to include acute rheumatic fever and some of the exanthemata—are associated with cold. The intermitting fevers—i. e. malaria and some cases of typhoid fever—plus cholera and the dysenteries, are associated with water. Tuberculosis and other chronic diseases of the lungs are associated with air. We have here a system of etiology, orderly, concise, unoriginal, vague, and incomplete, based in part on observations of seasonal and geographic distribution and in part on a tradition readily traceable through Boerhaave and Sydenham to Hippocrates.

In 1743 Colden wrote his "Observations on the Fever which prevailed in the City of New York in 1741 and 2" (31, 32). He stated that these fevers recur annually in the hot months. They are mild at the start of the season but grow steadily more malignant. ". . . they are at first commonly of the intermittent kind, but more frequently (especially as the hot weather advances) the paroxysms only remit, and at the same time many have continued fevers . . . the intestines are almost always affected, and have been found generally sphacelated in the bodies which have been opened . . ." This would seem to be the malaria-typhoid sequence, which embarrassed the medical profession until the final decade of the nineteenth century (33, 34, 35). It cannot be denied that the mélange may also have included cases of yellow fever and that Colden and other physicians had their doubts.

Colden's conception of the hot-weather fevers is a fair sample of the thinking of his era. Whereas in the essay previously cited (29) he had associated these sicknesses with the condition of local waters, he now speculated (32) that "the different state of the atmosphere, is the reason

of the different effects that the noxious vapors from stagnating filthy water have on the animal oeconomy; because, not only different kinds of vapours are raised from a different fermentation in the stagnating fluids, but they raise likewise different fermentations in the animal fluids; hence, different kinds of fevers produced in different constitutions of the air . . ." He observed that the part of New York City chiefly afflicted was built on a swamp. Offensive stagnation prevailed along the docks. House drains had been neglected. He now felt that fruit could be ignored as a cause of fever. He suggested that the construction of drains should thenceforth be at public expense and no longer depend on voluntary subscription. It is additionally significant that about half of his treatise is a paraphrase of Lancisi's classic *De noxiis paludum effluviis.*

We may notice with satisfaction that the officials of New York enacted part of the sanitary reforms which Colden suggested and tendered him a vote of thanks (36, 37). Following Biblical injunction they honored the physician, for they had need of him.

From the common hot-weather fevers of New York we may turn by easy transition to consider a dramatic disease which was later to burn its way into American history, viz. yellow fever.

To the casual or intermittent fancier of American medical history, yellow fever is associated mainly with the heroic efforts and monumental wrongheadedness of Benjamin Rush in the holocaust of 1793. From Rush's own statements (38, 39) we know that one major source of his ideas on yellow fever was a manuscript composed by the eminent physician, botanist, and cartographer, Dr. John Mitchell, F. R. S., of Urbanna, Virginia and transcribed for Benjamin Franklin. A copy of this manuscript reached Colden, who sent a reply directly to Mitchell. Mitchell's subsequent animadversions formed the third and fourth epistles in the series. Ultimately, in 1814, three of the letters were published by Hosack at the request of Rush (40, 41, 45). Both in its leisured pace and in its technical content the correspondence is typical of its era.

Mitchell's opening letter (40) described the yellow fever which prevailed in Virginia during 1737, 1741, and 1742. His information was derived from personal observation of patients and from five autopsies, at least one of which was performed on a slave. He defined the disease as "a pestilential fever proceeding from a contagious miasma sui generis, which inflames the stomach and adjacent viscera, obstructs the biliary ducts, and dissolves the adipose humours; to which generally succeeds an effusion of a bilious or other yellow humour upon the external or internal surface of the body . . ." Inflammation and spasms of the stomach and

other viscera hinder the flux of bile through the choledochus. The liver being obstructed there is a slowed efflux of blood into it and resultant stagnation in the omentum. The jaundice is due to the blood being dissolved. When the liver is inflamed or inspissation of bile occurs, a true icterus is produced. In newer terminology it would be said that both hemolytic and obstructive jaundice may be present. According to Mitchell the disease had been brought to Virginia twice by the warships of King George the Second.

Without entering further into the dismal swamp of eighteenth century pathogenesis and therapeutics, we may observe that Mitchell's conceptions were based on humors, which were made to operate according to iatrophysical principles, especially as these were promulgated by Sanctorius and Bellini. His essay contains elements of originality and is a vigorous, closely reasoned effort. He felt that venesection should be employed moderately but was insufficient as an evacuation. Reliance should be placed principally on sweating and purging.

Colden in his reply to Mitchell, June 8, 1745 (41), wrote " I have never seen any ill of the yellow fever . . . and therefore I can only speak speculatively of that distemper." He believed that the inflammation of the stomach occurring in yellow fever is not a true inflammation " arising originally from a stagnation of the globular parts of the blood in the capillaries " but is " entirely occasioned by an extraordinary acrimony in the bile . . . and that from this acrimony, and the solution of the bile as a consequence of it, the icterus or yellow colour ensues . . ."

Colden's speculations about yellow fever are a replica of innumerable pathologies of the previous seventeen centuries (42). He contended that in all fevers a proper degree of *velocity* (43) must be maintained. " If the velocity be too small, the noxious matter separates in the glands, whose excretories empty into the intestines and . . . re-enter the circulation. But if the velocity be too great, then no separation of the noxious matter can be made . . ." The safe method of regulating the velocity is not by evacuations but by giving vent to the fiery particles by a free perspiration. It is clear that Colden, like Mitchell, employed modified Galenic humors which were imagined to behave according to iatromechanical principles. The special attention to velocity may have been derived from Bellini or Newton or both.

Mitchell replied in an elaborate, important letter to Colden dated September 10 and 11, 1745 (44) and in a separate comment addressed to Franklin (45). He defended the purgative treatment of yellow fever and presented evidence that the disease was an importation.

In a subsequent letter to Mitchell, dated November 7, 1745 (46), Colden described two outbreaks of yellow fever in New York City. In the summer of 1743 the disease first became "epidemical" in New York. It began on the East River wharves in that part of the town called the Vly. This was low ground and had formerly been swampy. The high parts of the town remained entirely free, with minor exceptions. From these circumstances Colden concluded that though the sickness might not have had its origin in the putrid vapors of the wharf area it certainly owed its propagation to them. In June 1745 the disease appeared again, making its start upon wharves which had not been cleansed and drained. Again the high parts of the town escaped the disease. Despite the paucity of clinical detail and the possibility of confusion with malaria and typhoid, the epidemiological portrayal is compatible with the picture of yellow fever. Colden says: "From what I have with sufficient certainty learn'd I think I may affirm that the Infection was imported into New York both in the year 1743 & 1745 by its appearing first both times upon the Docks where the shipping lye & probably from the West Indies. In the next place I conclude that a putrid atmosphere is necessary for the propagating it . . ." He suspected that the several malignant fevers which had recently appeared in Europe and had received various denominations might all have originated from West Indian yellow fever; they had produced different manifestations according to different climates and atmosphere in the recipient countries. Fundamentally, however, all contagious fevers, such as smallpox, measles, and scarlet fever "have their specific levens or a kind of specific Genitura & which never alter no more than the species of Animals & Vegetables." After serious consideration he had found that the iatromechanical system was very defective and that alterations in the animal juices depend very little on mechanical principles. Thus yellow fever, like artificial smallpox, exposed the inadequacies of the iatromechanical system and forced the construction of newer hypotheses in which the thinking leaned toward what we to-day call biochemistry. It may be surmised, moreover, that the complex speculations which we have described were influenced by the general religious and theological tendencies of the era. Colden, Douglass, Lining, and Chalmers were born in Scotland. Colden was the son of a Scottish clergyman and was originally intended for the ministry, and Douglass was the typical disputatious Caledonian. The physiological hypotheses which they elaborated were hair-splitting rationalistic tractates, delivered with confidence, although without recourse to the supernatural. This resemblance between the pathology and the theology of the era is worthy of further exploration.

Another common element in the background of all these men was the influence of Newton. In the case of Colden this is perhaps of special importance, since Colden's major philosophical writings were an attempt to elaborate, develop, and correct the Newtonian conceptions of matter and energy.

Like the pestilential section of New York City, which had been built on a swamp, Colden's speculations concerning yellow fever in that city were erected on no solid ground of exact diagnosis. Hence it would be rash for the twentieth-century historian to impose a retrospective conclusion. Up to the time of writing I have been unable to satisfy myself that the disease complex discussed by Colden in his description of the annually recurring "hot-weather fevers" of 1741-42 (32) represents anything significantly different from the disease which occurred in New York in 1743-45 (46) and which Colden believed to be yellow fever. Unless this difficulty can be resolved we shall have no assurance that Colden's yellow fever was the same disease as Mitchell's. Colden reported to Mitchell that at the start of the 1745 outbreak in New York "the practitioners were abusing one another some positively asserting it to be the yellow fever & others denying it" (47).

The same vexatious obscurities complicate the problem of the so-called throat distemper of 1735-1740. On this subject we fortunately have the detailed and illuminating study of Dr. Ernest Caulfield (48), based on primary sources. Caulfield has shown that the "throat distemper" was an epidemiological complex. It included (a) an outbreak of severe diphtheria, which spread from Kingston, New Hampshire, to Maine and to Massachusetts, (b) an outbreak of diphtheria and mild scarlet fever in New Jersey, (c) an outbreak of diphtheria in Connecticut, which spread from Stamford in a northeasterly direction and in 1740 coalesced with the diphtheria of northeastern New England, and (d) an outbreak of scarlet fever in Boston, described by Colden's friend and correspondent William Douglass in one of the classic writings of early American medicine (49). Fragmentary evidence suggests that both scarlet fever and diphtheria were present in the province of New York at this time. The two diseases had not yet been clearly separated as independent entities but were fused and confused in the vague designation "throat distemper."

On this subject Colden's principal writing, which took the form of a letter to Fothergill, was characteristically the work of a theorist (50). He says "I have seen it [sc. the throat distemper] only in my own family, and in a few neighbours in the country . . . having entirely laid aside the practise of physic upwards of twenty years. What I chiefly learned was from the

late dr. Douglass, of Boston . . ." The disease as Colden portrays it is obviously a composite of diphtheria and scarlet fever; the separation of the intermingled descriptions is not relevant to our present purpose. Upon this double foundation Colden erected a superstructure of hypothesis. The humours of the body, he wrote, were divisible into three stages or classes. The first is that which circulates only within the larger ramifications of the veins and arteries and is properly called blood. The second is that which moves slowly in smaller ramifications than those which the red globules can traverse; here the sensible (perceptible) secretions are made. Third is lymph, which moves in still finer ramifications. This last is taken to be the principal instrument in the vital and vegetative functions of the animal. The animal food undergoes three different concoctions or digestions, after it has entered the circulation, in these different ramifications. By a fault in any one or more of these digestions, diseases of different kinds are produced. The seat of inflammatory fevers is in the first stage, viz. the blood. A fever arising in the second stage will be accompanied by disturbance of one or other secretion. The fevers commonly called nervous —this probably includes the modern typhoid and typhus—have their seat in lymph. From these principles it follows that in the first class the morbid matter may be most effectually removed by venesection, in the second class by one or more of the secretions, and in the third class by insensible perspiration only.

Colden felt that " those disorders which are commonly called nervous, whether acute or chronical, really proceed from some fault in the lymph; and that the distinction of lax and rigid fibres, is owing to an excess or defect of this humour, or to some other fault in it . . ." In modern terms this probably means that both typhoid fever (acute nervous fever) and chronic nervous disorders are attributable to quantitative or qualitative changes in lymph, this being the substance which permeates the finest interstices of the body, including those in nerves. Such an assumption makes it unnecessary to postulate changes in the tension of fibres as the cause of differences in symptoms.

We have in these speculations an attempt to classify febrile diseases according to sites of morbid action and to derive therefrom a rational method of treatment. Colden freely and frankly admitted that his clinical experience with throat distemper was small and that his main source of information was Douglass. Indeed, several phrases in Colden's communication to Fothergill are textually identical with passages in a letter written to Colden by Douglass in 1739 (51). The added hypotheses are obviously

Colden's; their historical origins would require a separate investigation beyond the compass of the present analysis.

Whatever its sources, Colden's essay gives us an excellent opportunity to observe the types of material, factual and theoretical, which this eighteenth-century physician utilized in his approach to the problem of an epidemic outbreak. Aspects mentioned in Colden's letter to Fothergill include: (a) geographical distribution—origin of the outbreak in an inland New Hampshire town, with alleged spread to the east bank of the Hudson River, later to the west bank, and at last over all of British North America (b) lack of foreign trade at the site of origin, evidence that the disease was not imported (c) the puzzling occurrence of detached cases, in which "it could not be conceived in what manner they could receive the disease by infection" (d) occurrence of the disease predominantly in children, i. e. what we of to-day would call "age incidence" (e) greater frequency of the disease among the poor than among the rich (f) greater frequency in rural than in urban areas (g) occurrence in pork-eaters (h) occurrence in persons of "scorbutic habit" (i) occurrence in dwellers on wet ground (j) occurrence of subsequent minor outbreaks, which showed little tendency to spread (k) the strictly clinical appearances and their variations (l) the findings in a few autopsies (m) the effects of treatment (n) an effort to integrate these elements into an explanation of the throat distemper and into a general theory of febrile disease.

What we chiefly miss in the studies of Colden and Douglass is a body of facts acquired by experiment. I have already demonstrated that if Colden theorized copiously in medicine he theorized somewhat reluctantly, and he was aware both of the value of experiments and of their possible weaknesses. But the experimental study of infectious disease was yet to flower.

Even in the Colonies, remote as they appeared to be from the intellectual centers of Europe, contemporary activities in experimental medicine were not unknown. We have previously discussed Colden's experience with variolation (*27*). It is evident from a letter of Robert Whytt (52) that Colden was aware of the experimental transmission of measles by Francis Home. We know from a letter of Alexander Garden (53) that Colden had at least heard of Lind, whose work on scurvy hinged on a controlled clinical experiment. In addition there is abundant evidence of American interest and participation in the activities of the Royal Society. Thus at least a groundwork of informed opinion had been created.

In preceding paragraphs of this study I have attempted to define Colden's attitudes toward the infectious diseases and to describe his views

on the special subjects of variolation, the hot-weather fevers, yellow fever, throat distemper, and experimental medicine. Completeness requires that brief mention be made of a few incidental observations and opinions which are scattered through his correspondence.

Like many a physician and historian, Colden meditated on the origin of syphilis. His conclusions, expressed in letters to Mitchell (54) and Gronovius (55), were that, contrary to the opinion of Sydenham, syphilis and yaws are different diseases, that both are contracted by infection, that syphilis was probably present among the Indians before the arrival of the white man, that yaws was introduced from Africa and consequently could not have existed in America in pre-Columbian times, and that "tho they may both be properly placed under the same Genus they are specifically different & require different Methods of cure."

The caveat which we have entered against Colden's diagnosis of yellow fever applies also to yaws. Until a large number of case reports and detailed descriptions has been accumulated and analyzed we shall lack justification for assuming that the yaws discussed by Colden is the same as the yaws which still prevails in the Antilles. It would be rash to undervalue Colden's understanding of yaws, since he may have had experience of sibbens, the endemic treponematosis of Scotland (56, 57). For the solution of this specific problem useful information might be obtained from paleopathology (58). *Mutato nomine de* quo *fabula narratur?*

To the twentieth-century student it is astonishing to see scurvy included in a discussion of infectious diseases. Colden's comments, written to Mitchell ca. 1745 (59) and to Kalm in 1751 (60), are unhappily an evidence of the confusion and vagueness which Lind was soon to deplore. Scurvy, said Colden, is exceedingly common in North America and is often mistaken for other diseases because of the different shapes which it assumes in this climate. Hardly a family in this country is free from the disease. It causes widespread loss of teeth among the European but not the Indian inhabitants.

> The far greatest number of the Inhabitants when brought over to America were mean people crowded in ships & run with the Scurvy when they landed & great numbers of Such are yearly imported In former times they had little fresh Provision or Sallad . . . And I take the Scurvy in a considerable degree to be contageious when people sleep together it is no wonder that it is become endemick The Rheumatismus scorbuticus being very common in this country is another proof of this . . .

It would seem from this statement that Colden's concept of scurvy included cases of dental caries, pyorrhoea, arthritis, and perhaps also cases of typhus and leukemia.

The materials considered in the preceding pages make it possible for us to define Colden's reactions to the infectious diseases which prevailed in his time and region. He was a physician of sharply limited clinical experience. Endowed with high talents as a naturalist and a philosopher, gifted with great energy, and blessed intermittently with geographic isolation, he turned his inquiring mind to the construction of systematic hypotheses, which were developed as part of the effort to understand the phenomena of disease. That such philosophical systems were characteristic of the era is well known. From his teachers and from the reading of such authors as Santorio and Bellini he inherited the iatromechanical philosophy. The facts of clinical medicine reached him chiefly through wide reading and extensive correspondence. Largely because of apparent anomalies in the behavior of infectious diseases, he came to perceive the inadequacies of viewing man as a Bellinian machine-shop of levers, pulleys, bellows, and pumps. Under pressure of new facts in what are to-day designated immunology and epidemiology, his conceptual universe was expanded—not abandoned—and came to include a recognition of the similarity between the phenomena of fevers and those of fermentation. This priceless philosophic intuition, which Colden appears to have derived directly from Lancisi (61), and perhaps indirectly from Fracastorius (62), was to reappear in the work of Rush and more effectively—*multas per gentes et multa per aequora vectus*—in the inspired researches of Pasteur.

Another valuable intuition, concealed in an anonymous note which Colden sent to the *Gentleman's Magazine* (16) concerns an analogy which he surmised "between cancers and the tumours made by some insects, laying their eggs in leaves, or the bark or fruit of vegetables, and in the flesh of animals." The validity of this surmise is well known to phytopathologists who have dealt with the problems of crown-gall and of endothia canker of chestnut (63).

Colden's ratiocinations in the sphere of medicine were carried to no extreme. They produced no sects or schisms or famous discoveries and they caused no martyrdom. Yet we are obliged to believe that his constant rethinking and repeated discussion of important themes was a stimulus to active workers in the field.

In a memoir attributed to Gulian Verplanck (4) the life work of Colden is thus summarized:

> When it is considered how large a portion of his life was spent in the labours or the routine of public office, and that however great might have been his original stock of learning, he had, in this country, no reading public to excite him

by their applauses, and few literary friends to assist or to stimulate his inquiries, his zeal and success in his scientific pursuits will appear deserving of the highest admiration . . . A mind thus powerful and active could not have failed to produce great effect on the character of that society in which he moved; and we doubtless now enjoy many beneficial, although remote effects of his labours, without being always able to trace them to their true source.

Acknowledgement

The author desires to record here his appreciation of assistance received from the learned ladies and gentlemen of the libraries of the New-York Historical Society and the New York Academy of Medicine.

REFERENCES AND NOTES

The best available primary source of biographical information about Colden is his meagre letter to Kalm (2). Secondary sources are replete with error.

The Colden papers are in the New-York Historical Society (19, 20, 21). The materials published by the Society do not include the purely scientific and philosophical treatises written by Colden (see Colden Letters and Papers (21), Vol. 1, p. ix) but these are listed by title (*ibid.*, (21), vol. 7, pp. 359-376).

Inasmuch as no complete Colden bibliography has come to light during the present researches, and in view of the fact that the diversity of Colden's work has led to the publication or republication of various of his writings in treatises not ordinarily consulted by students of the history of medicine, the following entries have been extended somewhat beyond the limits which are customary in a work of this character. It is hoped that future scholars will profit thereby.

Those who seek supernormal completeness will be well advised to make a careful survey of the periodical press of America, England, and Scotland, with special attention to anonymous articles, which can be checked against hints derived from the Colden Letters and Papers (21) and against manuscripts in the New-York Historical Society. It was this method which led to the detection of the articles on tar water (64) and cancer (16, 17).

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GIOVANNI RASORI (1766–1837)

OR

ITALIAN MEDICINE IN TRANSITION [1]

ENNIO ROSSI

The writings of Giovanni Rasori contain numerous indications of his indebtedness to the Scotch systematist John Brown. For this reason he is considered essentially a Brunonian. That Brown did exert a great influence on Rasori is incontestable, but a careful analysis of Rasori's works, especially of those written in his later life, reveals many progressive ideas that tend to modify the above conclusion. The purpose of this paper is to review Rasori's theories, and from the perspective thereby obtained, re-evaluate his position in the history of medicine.

Giovanni Rasori was born in Parma, August 20, 1766. From his father, chief pharmacist at the hospital of that city, he inherited a great aptitude for science. He attended the medical school at Parma, graduating at the age of 19. Michele Girardi, his professor of anatomy and a former student of Morgagni, introduced him to Count Giuseppe Camuti, chief physician to the Court of Parma, who in turn brought the promising young man to the attention of his sovereign, Duke Ferdinando di Borbone.

Under the patronage of the Duke, Rasori studied at the foremost medical schools of Europe. He first went to the University of Florence in 1788. Through Michelangiolo Gianetti, the professor of anatomy, he had his first contact with the teachings of the Scotch systematist Brown and began a translation of one of his works. In 1791, he went to the University of Pavia, then enjoying its greatest fame with men such as Scarpa, Volta, and Spallanzani. His translation of Brown, published in Pavia in 1792, brought him widespread recognition, especially for its very scholarly "Discorso Preliminare." In 1793, he went to London where he remained until February 1795. There his training in Anatomy, started under Gianetti at Florence and Scarpa at Pavia, was continued under the disciples of Hunter. He returned to Italy in the spring of 1795.

Stirred by Napoleon's First Italian Campaign, in 1796 Rasori became

[1] This article is an M. D. thesis deposited in the Library of the University of Wisconsin. It is my pleasant duty to acknowledge my debt and express my gratitude to Dr. Erwin Ackerknecht who suggested the topic and guided the investigation for this paper. I am also indebted to Prof. Joseph Rossi for valuable suggestions and criticisms.

active in politics. The French victory at Lodi on May 10, and the subsequent occupation of Milan (May 15) gave Napoleon the control of Lombardy, and the devotion of the Italian liberals who hailed him as the liberator of Italy. It also placed the Italian liberals in power in the newly formed Cisalpine Republic. In the reorganization of the university faculties, Rasori was appointed (November '96) professor of pathology at the University of Pavia, and rector of Ghislieri College.

In his opening lecture,[2] delivered on January 9, 1797, Rasori expounded his medical philosophy. He condemned the Dogmatists for believing in strict doctrines without benefit of investigation and the Empirics for not formulating concepts from their observations. He concluded with a statement affirming the importance of analyzing and organizing facts into a logical pattern. He praised in particular Brown for having done this, thereby constructing an organized science from a mass of isolated facts.

> In conclusion: no branch of knowledge can pretend to its fullest degree of perfection and utility, unless its facts are organized and brought to the state of a science. The philosophical analysis of these facts and the derivation of simple, immutable general principles is what constitutes a science. Without analysis and without conclusions based on all the facts in their proper perspective, the unrelated knowledge of the facts themselves constitutes only a mass of obscurity, hindering more often than helping any practical application.
>
> Brown has been the first to establish Medicine as a science: and hereafter physicians will make use of it as such, in spite of the Empirics, praisers of observation, who will no longer find in us the uncritical blind faith which they have demanded and obtained till now.[3]

After a brief absence of nine months, during which he served in Milan as Secretary General of the Ministry of Interior, Rasori returned to Pavia as professor of medicine. In this new post, he placed himself in an extremely difficult position by expressing unpopular opinions on both Hippocrates and Brown. His first attack was a lecture given before the student body entitled "The Supposed Genius of Hippocrates." He vigorously condemned medical tradition for the high standing it had accorded to Hippocrates—a typical pre-romantic reaction against Eighteenth Century classicism:

> For many centuries, Hippocrates has been looked upon as an oracle; his writings, his words, all his works have been held as oracles, almost as the gospel of human reason. The critical days, the four-fold division of the humors, the derivation of

[2] G. Rasori, *Prolusione*, Milano, 1797.
[3] *Ibid.*, 31-32.

disease from dryness and humidity, and many other doctrines founded in Cos have corrupted medical thought for countless centuries. One cannot deny, that there are some truths found in the chaos of his works, but they are, as it were, drowned among hundreds of trifles and absurdities, ***rari nantes.***[4]

He concluded with the following evaluation of the scientific contribution of Hippocrates:

> But if he is to be considered the father of anything in medicine, it is of all the errors he was the first to disseminate and which have grown robustly for many centuries. It is of the blind, superstitious deference to antiquity, which, through the disciple's devotion to the master's word, has retarded the progress of science for centuries. It is of the many absurdities that came from the minds of his commentators. It is of the verbose disputations and interpretations in which were lost the intellects of his followers.[5]

He was not, however, content with this blast at Hippocrates. His opening lectures in medicine expounded much of his "Brunonian reform," advancing theories that were in direct opposition to Brown. Since, at that time, Brown was very popular in Italy, his position alienated him not only from admirers of Hippocrates, but also from strict Brunonians.

This hostile reaction spread also among the students. A group of them filed a petition with the executive council of the Republic, charging Rasori with lack of respect for scholarship and medical tradition and demanding his dismissal. They also submitted a poetical skit entitled "Il Rasori," ridiculing the treatments based on his Brunonian reform. The opening scene of this "play," set in Rasori's medical wards, shows the patients complaining bitterly over the treatment prescribed by their "physician"—nothing but tartar emetic, bleeding, and starvation. However, in the last scene, during the physician's absence, the patients rebel against the treatment, eat all they want, and, of course, immediately recover from all their ailments.

Rasori's biographer, Del Chiappa, suggests that Pietro Moscati instigated the student demonstrations against Rasori.[6] Moscati, who had been the professor of medicine, had resigned in order to accept a post on the executive council of the Republic. However, he soon found politics distasteful, and longed for his former post at Pavia which Rasori now held. Moscati may have instigated the students' activities, but it is unlikely that devious methods were necessary to remove a man who had succeeded in

[4] G. Rasori, "Il preteso genio d'Ippocrate," quoted in Giuseppe Del Chiappa, *Della vita di Giovanni Rasori*, Milano, P. A. Molina, 1838, pp. 51-52.

[5] *Ibid.*, pp. 53-54.

[6] G. Del Chiappa, *op. cit.*, p. 46.

alienating such a large group of his colleagues. In any case Moscati returned to Pavia in January 1799, and Rasori was sent to Milan to assume an administrative post in the Ospedale Maggiore.

Rasori did not remain long in Milan for, with the formation of the second allied coalition in December 1798, and the renewal of the European war in 1799, the French were driven out and Lombardy was again under Austrian rule. Rasori fled to Genoa where he served as a doctor with the French army. While he was in Genoa, there was a severe outbreak of petechial fever (apparently typhus) among the populace. Rasori made a study of this epidemic, and in 1800 published his *Storia della febbre epidemica di Genova negli anni 1799 e 1800.*

In July 1800, after Napoleon's second Italian campaign had been brought to a successful conclusion at Marengo and the French were again in power in Lombardy, Rasori returned to Milan. He was offered the post of Minister of the Interior but declined, accepting instead the office of Inspector General of Health, and later that of chief of the military and civil hospitals of Milan. These administrative posts took much of his time from scientific studies, for he headed many commissions for the government, investigating medical facilities throughout the province. However, he did edit a new medical journal, *Annali di Medicina*, which had its first issue in July 1802. But at the end of that year Rasori discontinued its publication, being unwilling to place it under government censorship. He then devoted himself anew to a project he had begun in 1799, the translation of Erasmus Darwin's *Zoonomia*. The first volume of the translation appeared in 1803, and the work was brought to completion two years later with the publication of the sixth volume.

As Inspector General of Health and chief of the military and civil hospitals of Milan, Rasori established his precepts of therapeutics in the major hospitals of the city, and perhaps the province. His reformation of Brown led to his adoption of a system of therapy, differing greatly from that of Brown and other physicians following the strict Brunonian method. This fact, coupled with his overbearing personality, often made him the subject of heated controversies, and in 1806-1807 his therapeutics came under sharp attack by his colleagues, especially his use of antimony. Rasori rushed to his own defense, and the polemic soon grew to large proportions. The Minister of the Interior was about to pass a decree against the use of antimony, when a study of mortality figures in all the hospitals of Lombardy showed that both the civil and military hospitals of Milan had a comparatively low mortality. This apparently settled the argument in favor of Rasori once and for all. In fact, shortly after, he was made Direc-

tor of Public Instruction, and with the establishment of a new medical school at the Military Hospital of S. Ambrogio, he was made chief of its teaching clinics. His reputation rose considerably with such men as Tommasini, Bufalini, Brera, Borda, and others numbered among his followers. He won recognition abroad as well. Even the great Laennec, in his use of antimony, followed the Rasorian treatment for pneumonia.[7]

However, Rasori's position of influence was brought to an abrupt end in 1814, with the fall of the Napoleonic Empire and the reestablishment of the Austrian rule in Lombardy. One of the first acts of the new regime was the exclusion of "foreigners"[8] from public office. This stripped Rasori of his influential posts and forced him into private life. Shortly after he became involved in a plot against the Austrians. The plot was discovered, and on December 4, 1814, Rasori was arrested and sentenced to prison.

In prison Rasori turned his attention to literature. It was not a new interest for him, inasmuch as he had collaborated with the great poet and patriot Ugo Foscolo in the publication of the journal *Annali di Scienze e Lettere* in 1810. Now, in the enforced leisure of prison life, he turned his attention to the mastery of German, being already quite conversant with French and English. He translated into Italian a number of poems by Wieland, Goethe, and his favorite author Schiller, many of them later printed in Milan newspapers. He also translated *Agatocle* by Karoline Pichler, a forerunner of the historical novelists of the romantic period, so popular at that time. However, his most ambitious work was his two volume translation of Engel's *Ideen zu einer Mimik* which he published in 1819. It was an unusual translation in which he took the liberty of inserting many passages from Dante, Petrarch, Ariosto, and other Italian masters, wtih the specious reasoning that if Engel had been Italian, he would have used those passages to illustrate his ideas.

After his release from prison in 1818, Rasori joined the literary group which published *Il conciliatore*. Because of its liberal tendencies this journal was suppressed by the Austrians the following year. Rasori, probably tired by the constant struggle his life had been, then retired to a quiet life, and lived in virtual obscurity until 1830. In that year he published, in two volumes, his *Opuscoli clinici* (Clinical Pamphlets). In 1833 he came out with another volume, a biography of Erasmus Darwin. It

[7] C. Fiessinger, *La thérapeutique des vieux maitres*, Paris, Société d'Editions Scientifiques, 2nd. ed., 1897, p. 341.

[8] Being a native of Parma, an independent duchy, Rasori was a foreigner in the Lombard-Venetian Kingdom.

was not, however, until 1837 that his more famous book, *Teoria della flogosi*, was published. He started work on this volume in 1831, compiling all the work he had done on inflammation since the turn of the century. Rasori did not live to see his *Teoria* published, for on April 13, 1837, he died of pneumonia. His burial was in the church of S. Giorgio in Milan. With his death, clinical medicine in Italy found itself, for the first time, without a man of international repute numbered among its ranks.

Rasori's Brunonian reform consisted in his theory of *controstimolo*. Before any detailed discussion of this concept, we should first say a word about Brown's original theory in order to appreciate fully the different points of view of these two men.

Brown's theory rested on the assumption that living matter possessed a vital irritability, and that all active phenomena of life were the result of the action of stimuli on this irritability. In other words, life was simply a state of excitation—the living body having a fixed and uniform capacity to respond, the variation in response determined by variations in the stimuli. From this basic premise, it was but a short step to his definition of disease as the result of disproportionate amount of stimuli, either too much or too little, acting upon the living body. This concept led to his classification of diseases as sthenic (too much stimulation), or asthenic (too little stimulation). The latter group he further subdivided into two groups, direct and indirect asthenia. Direct asthenia was caused, as already stated, by too little stimulation, while indirect asthenia was due to stimuli of such magnitude that the resulting sthenic response quickly exhausted the body and brought about an asthenic state. An example of the latter classification is the effect of emetics, which leaves the living body in a state of asthenia due to the violence of the vomiting.

Although Rasori was an enthusiastic supporter of Brunonianism, he did not accept Brown's monistic explanation of life and disease. He was willing to admit that sthenia was caused by excessive stimulation, but he rejected as sophistic the definition of asthenia as merely a lack of stimulation. To show the inadequacy of this classification of asthenia, he cited numerous examples of diseases which did not fit into either the category of direct, or indirect asthenia. One of these was malaria.

> This brings to our attention a certain type of intermittent, pernicious fever, produced, as they say, by bad air, which is so prolonged and violent, that it cedes only to the strongest stimuli. This said, one may ask what is the effect of bad air on the system. Is it stimulating or debilitating? First of all, it is certain that the diathesis of these fevers is not sthenic, as evidenced by the exclusively stimulating

method of cure it requires. It cannot be an indirect weakness, since no one has ever observed a sthenic diathesis at the moment of exposure to bad air, and it is inconceivable to imagine an indirect weakness without the manifestation of a degree of sthenia, at least at the incipience of the stimulant's action—something that occurs with any other known stimulant taken in a large dose. Therefore the weakness brought about by bad air is not indirect. It remains now to be seen whether it can be considered direct.

It is the rule of direct weakness that the more advanced is the weakness, the less intense is the stimulus needed to counteract it, because of the accumulation of irritability. However, in the case of this fever produced by bad air, the longer the weakness is extended and the greater the symptoms, the stronger is the stimulus required to fight the fever and forestall its onset. This contradicts the treatment of direct weakness. We have seen, therefore, up to this point, how the emetics, purgatives, and bad air are able to induce a weakness which is not reducible to either the direct or indirect of Brown.[9]

To fill this gap he created in Brunonianism, Rasori formulated his theory of *controstimolo*, which he first presented to the medical public in 1798 when he was professor of medicine at Pavia. This theory postulated the existence of what he termed "contra-stimuli," hypothetical substances with an action diametrically opposed to stimuli, causing a diminution of excitability and, consequently, an asthenic condition.

After having stated the principal facts that reveal as false the Brunonian principle that everything acting on irritability acts by stimulation, let us go on to establish another type of action on living tissues—an action that is diametrically opposed to that of Brown's stimulus. Substances possessing this action, when applied to the system, have the faculty of diminishing irritability, but not by either removing stimulus or being excessively stimulating. This action is found in substances such as prussic acid, cherry laurel water, nux vomica, and many others. This action I call contrastimulation. Through a principle of activity of its own, which is as yet unknown, it stupifies the tissue, making it less irritable and less sensitive to the usual stimulants. A contrastimulating action therefore indicates an action completely opposite to that of stimulation.[10]

With the substitution of the term diathesis of stimulus for sthenia, and the development of this concept of *controstimolo*, which he termed diathesis of contrastimulus, we have what essentially constituted Rasori's Brunonian reform.

Although Rasori's departure from Brown was only slight in theory, in therapy the difference was quite marked. Brown felt that the majority of illnesses were asthenic, or in Rasori's term, had a diathesis of contrastimulus, and therefore he used mostly stimulants. Rasori, on the other

[9] G. Rasori, "Teoria del controstimolo," in G. Del Chiappa, *op. cit.*, pp. 30-31.
[10] *Ibid.*, p. 38.

hand, felt that most diseases, approximately 97%, were inflammatory and had a diathesis of stimulus, requiring a contrastimulating treatment. Therefore his treatment consisted essentially of copious bleeding (as much as four and one-half liters in four days!)[11] plus great quantities of heavy metals and purgatives.

Rasori did not utilize auscultation or percussion. His diagnostic procedures consisted of a trial run of bleeding, an antiphlogistic or "contrastimulating" remedy. If the response was good, he postulated the presence of a diathesis of stimulus and continued giving depressing or "contrastimulating" remedies; if the response was poor, he decided the patient had a diathesis of contrastimulus and discontinued the bleeding, starting treatment with stimulants. The extent of diathesis present was referred to as *capacita morbosa*, and was determined by the degree of response to the trial run of bleeding. This in turn determined how much counteracting therapy should be used. All these values, however, were obtained through clinical judgment with no objective criteria whatsoever. Rasori's use of drugs in both diagnostics and treatment, and his general therapeutic management are well illustrated in the following report of a case of pneumonia.

. . . I prescribed a bleeding and six grains of tartar of antimony, to be taken during the night. That was the usual prescription I ordered in all cases of this nature, so as to explore the force of the diathesis of stimulus. The next day the nurse reported that the patient had been seized by a violent hypercatharsis, appearing much like cholera, with convulsions and fainting spells, which made him fear the patient would not live through the night. In fact, I found the patient with a cadaverous expression, his forehead covered with a cold sweat, his extremities cold, and his pulse almost imperceptible. I then noticed that the patient had taken only a third of the tartar of antimony I had prescribed—two grains. The nurse had discontinued the drug, because the vomiting and diarrhea had augmented to such a point, that it would have been impossible to continue its administration . . . It was therefore evident that the diathesis was different from what we had supposed at first. Later this conclusion was confirmed by the comparative study my students made between this patient and others who were in the hospital at the time with pneumonia. Tartar of antimony did not produce an evacuating effect in any of them, although it was administered in doses of 20 and 30 grains . . . After having well weighed these circumstances, I did not feel that I should persist in the treatment begun, nor that I should remain with crossed arms under the safeguard of expectant medicine. I instead had recourse to opium, of which I prescribed six grains to be taken with a strong wine, in six hourly doses. I then made arrangements with the surgeon on my service, so that every two hours, one of us visited the patient

[11] A. Pazzini, *Storia della medicina*, vol. II, Milano, Societa Editrice Libraria, 1947, p. 226.

and examined the changes that had taken place. The result was what we expected: the patient responded very well to the opium and wine, and proved thus that the *capacita morbosa* was of contrastimulus and very intense.[12]

Despite the obvious Brunonian origin of the theory of contrastimulus, Rasori's *Teoria della flogosi* gives us a different perspective. In this work he deals with the very basic problem of inflammation, utilizing a scientific approach. Rasori felt that pathological anatomy was very important from the scientific as well as the clinical viewpoint. It was important for the understanding of inflammation *per se*: "Inflammation, as a morbid function of the organ it attacks, can be understood in its nature and in its effects only through the anatomical study of the organ in which it is present."[13] Later on, in expressing the hope that, "pathological anatomy will give, to both the art and the science, contributions of greater importance than it did in the past," he pointed out that, "To do this, it must be principally applied to post-mortem studies aimed at ascertaining the actual effects of curative methods employed, thus showing the physician the correctness or error of his diagnostics and treatment."[14] For these reasons Rasori performed many autopsies, especially between the years 1797-1814, and somewhat fewer up till 1835, before he wrote the *Teoria*. It was these post-mortem studies which formed the basis for this work.

The *Teoria della flogosi* was divided into two volumes. The first volume was devoted to a discussion of the normal components of the blood and of the previous work done on inflammation by other men. The second volume contained his own observations and conclusions on inflammation. It is this second volume with which we are primarily concerned.

The simultaneous presence of purulent exudate and vascular engorgement (a reiteration of the correlation Hunter had attempted between inflammation and thrombophlebitis) was one of his most frequent observations. As an illustration of this point, Rasori included the description of an autopsy which he performed on a fatal case of empyema. The following, a quotation of this description in its entirety, serves us as an excellent example of his autopsy technique.

After closely examining the internal surface of the thorax, which contained a great deal of viscid purulent material, I had it very carefully cleaned with water. Then we could see the costal pleura covered with capillary vessels, both large and small, engorged with blood, and arranged in a network; in a word, it was an

[12] G. Rasori, *Théorie de la phlogose* (French translation by S. Pirondi), Paris, 1839, II, 257-259.

[13] *Ibid.*, I, 7-8.

[14] *Ibid.*, I, 300.

inflammatory capillary network of great thickness. Since the lung could not be seen, my assistants were convinced that it had been decomposed and entirely reduced to the purulent material that filled the thorax . . . However, I examined closely the left thoracic cavity, and observed that the vertebral column deviated a little to the left describing a slight curve which gradually diminished at the superior and at the inferior portions of the thorax. I concluded that this was the lung, pushed against the vertebral column by a strong compressing force. . . . In fact, in observing this "enlargement" of the vertebral column more closely, I noted that it was elastic. I then examined it with the lens and saw a surface covered by a large number of small buttons no larger than the head of a pin which resembled papillae. It was a pseudo-membrane. . . Using the flat side of a knife, I succeeded in carefully detaching it from the vertebral bodies to which it was adherent. Although it tore in some places, I was able to lift it completely from the spot where it was attached. We found the lung underneath this pseudo-membrane, but it had become so small that it measured no more than six or seven finger breaths in length and two or three in width. Although reduced to such a small volume, it had not lost an atom of substance, the pleura being perfectly intact. At a distance, the color of the lung seemed very dark; closer, one could see the pleura covered by a very fine, closely set, capillary network which undoubtedly caused this dark red color. The pseudo-membrane was approximately one-third *ligne* (*ligne*—$\frac{1}{12}$ inch) in thickness. It was smooth on its inner surface and branched here and there by capillary vessels engorged with blood. Wishing to ascertain whether there were any alterations inside the lung, I had air introduced with the aid of a tube pushed into the trachea. More than half of the superior portion dilated very easily. In cutting it, we found it engorged with black blood, especially inferiorly. None of the pulmonary parenchyma had been destroyed, indicating that it had not contributed in any way to the formation of the purulent fluid. The blood vessels, the bronchi, and the cellular tissue uniting them were perfectly intact. The ease experienced in insufflating the lung indicated that a good part of the bronchial tree was also intact. As to the inferior portion, which was certainly smaller, the resistance shown to insufflation indicated vascular engorgement and the beginning of hepatization. This portion of the lung was therefore equally solid, not having any internal lesions—any cavities, or, in a word, any solution of its continuity.[15]

At this point Rasori deviated from his main thesis in order to make an important point concerning the treatment of empyema as indicated by these post-mortem findings. This illustrates again his application of pathological anatomy to the practice of medicine.

In this case, two very grave circumstances developed. The abundance of the semi-fluid material which occupied a great portion of the thoracic cavity, and the pseudo-membrane which did not permit the lung to resume its functions, were circumstances which would have rendered completely useless the operation for empyema, if it had been attempted.

Finally, I recommend to any surgeon who might be consulted (in cases identical

[15] *Ibid.*, II, 109-113.

or analogous) on the applicability of the operation for empyema, not to wait too long before performing this procedure. Although this operation has only a slight possibility of success, if not attempted the patient is sure of a certain death. And a possibility of success, as slight as it may be, must always be preferred to the certitude of a fatal and prompt termination. This conclusion is irreproachable.[16]

Having shown in this autopsy record and numerous others that the formation of pus was not due to necrosis of the diseased organ, and having shown also the presence of dilated blood vessels at the site of inflammation, Rasori concluded that the inflammatory exudate originated from components of the blood. He ruled out red blood cells as components of purulent fluid, taking into consideration its typical milky white color, so that "even when red blood cells are found in an area of suppuration, they do not enter into the formation of pus, but remain there as a heterogenous part."[17] He concluded therefore that serum and "fluid" fibrin, "through a process of extravasation, abandoning the capillaries of an inflammatory network"[18] were the only components of the blood directly concerned in the formation of purulent matter. He was cognizant of the work done on the microscopic examination of pus. He mentioned with interest the reported discoveries of "microscopic spherical molecules" in the purulent exudate[19] but did not realize the importance of these "molecules" and dismissed them as oddities. In general he felt that microscopic examinations were a fruitless effort.

One can see in Rasori's last work a utilization of scientific observation and a fortunate lack of the *a priori* reasoning so prevalent in his theory of *controstimolo*. He successfully avoided the intellectual trap of attempting to evolve a cosmic concept of disease—a trap that was his previous undoing as well as that of Brown and the rest of the systematists. His conclusions were entirely based on unprejudiced observations. This was a great improvement over his previous practise of subjecting observations to a projection *ad absurdum*, in order to prove a preconceived conclusion —a practise which resulted in philosophical abstractions rather than scientific conclusions.

The true worth of this book can probably be best seen in the criticisms of his contrastimulist followers, one of whom was Del Chiappa, his biographer.

The theory of phlogosis is not a complete work; or it is complete, as far as can

[16] *Ibid.*, II, 114-115.
[17] *Ibid.*, II, 140-141.
[18] *Ibid.*, II, 142.
[19] *Ibid.*, II, 150-151.

be, considered from a certain aspect. It lacks a theory integrating the facts; it lacks a doctrine of the genesis of morbid inflammation; it lacks the investigation of the causes producing it and, what is more important, it lacks the investigation of the diathesis . . . Furthermore it has two additional defects, in my opinion. First, it strove to form general rules, and constant dogmas in things where they do not obtain, namely the pathological state of viscera in connection with the signs of inflammation. Second, it did not evaluate sufficiently the vital force, falling into the error of Spallanzani in respect to digestion, which reduced all pathological functions to a pure and material mechanism.[20]

In all probability, this is the best commendation the book could have received.

Of far greater interest is Rasori's work on malaria. We have seen that Rasori at first considered malaria a disease with a diathesis of contra-stimulus. However, in 1816, while in prison, Rasori himself contracted malaria, as did many of his prison mates. Taking full advantage of this excellent opportunity to observe the disease, he began to question seriously all his previous beliefs. In a letter to his close friend and former student Fossati, he stated that he had considered and solved the following questions about intermittent fever. "Does bad air actually cause malaria? Is the phenomenon of intermittence due to a factor in the human body or to external factors? What part has the diathesis in this disease? And if no part at all, what then causes the morbid state of the intermittent fevers? And how does quinine act in curing malaria?" [21] He made the flat statement that quinine and only quinine was to be used in treating malaria, thus retracting his previous denunciation of that drug.[22] He did not wish, however, to divulge this new information, and he earnestly entreated his former student not to reveal to anyone the contents of the letter.

It was not until 1846, nine years after Rasori's death, that his remarkable insight into the disease of malaria was made known. In that year Agostino Bassi, the founder of the doctrine of pathogenic microorganisms of vegetable origin, and the forerunner of Schwann, Pasteur, and Lister,[23] published a pamphlet in which he referred to a conversation he had had with Rasori.

As soon as Rasori heard of my thesis explaining the nature of *mal segno*, he said to me: "I am fully convinced of the truth of your useful discovery: For many

[20] G. Del Chiappa, *op. cit.*, pp. 177-178.

[21] F. Marimo, "G. Rasori e la malaria," in *Rendiconti dell'Associazione Medico-Chirurgica di Parma*, 1900, p. 164.

[22] G. Del Chiappa, *op. cit.*, p. 88.

[23] W. Bulloch, *History of Bacteriology*, London, Oxford University Press, 1938, p. 159, 161.

years I have held the opinion that intermittent fevers are produced by parasites, which renew the paroxysm by the act of their reproduction, which recurs more or less rapidly according to the variety of the species. And thus would be caused the intermittency of the fever—quotidian, tertian or quartan." [24]

The accuracy of this statement becomes even more startling when one realizes that this conclusion was probably arrived at in 1816 in the Mantuan prison—a full sixty-four years before Laveran demonstrated the malarial parasite microscopically.

In the same pamphlet Bassi refers to another conversation with Rasori, which reveals the latter's whole hearted acceptance of the germ theory of disease.

One day I had a long discussion with Rasori concerning my opinion that all contagious diseases are produced by living beings, animal or vegetable parasites. After this discussion, which lasted three hours, he said to me as I was leaving, "If anyone else had heard our discussion they would have exclaimed—There they are! Those two madman got together." If my dear friend were still living, he would be very happy to see verified little by little as fact, that which was thought at one time, and which is still thought by many, to be nothing but overheated imagination.[25]

In reviewing the theories of the Eighteenth Century systematists, one is struck by the appalling waste of intellectual power over theories already buried and exhumed countless times. Brunonianism was no exception, for one can see its precursors in the second school of cosmogony of Ancient Greece.

This sect of philosophers, led by Democritus and Epicurus, were the materialists of their day who used the laws of matter to explain all natural phenomena. It was the direct application of their atomic theory that formed the foundation of the medical theories of the Methodists.

The Methodist School had its beginnings with the teachings of Asclepiades (*circa* 80 B. C.), the foremost anti-Hippocratic physician of his day. Adopting the atomic theory of Epicurus, he believed that atoms of various size passed constantly back and forth through the pores in the human body. So long as a normal relationship, in respect to size and proportion, existed between the pores and the atoms, health was the result; when this balance was upset, the result was disease. On this basis the Methodists developed their doctrine of *strictum et laxum*, which considered the solid parts of the body (ancient solidism) as being either constricted or relaxed. They viewed vital reaction merely as a secondary

[24] F. Marimo, *op. cit.*, p. 165.
[25] *Ibid.*

phenomenon, dependent on changes in material substances, and resulting from the passive response of the living body, in direct opposition to the Dogmatists who considered vital reaction a primary phenomenon. This concept reflected itself in the Methodist therapy. Believing that vital reaction was merely a passive state, they felt that cures would result only from an active intervention in the disease process, and rejected completely the belief in the healing powers of nature held by the Dogmatists.

With slight variations, this concept reappeared many times. Stahl (1660-1734) substituted his "sentient soul" for the Methodists "laws of matter" but he still assumed that the body was only a passive machine. Hoffman (1660-1742) postulated diseases as being atonic or tonic, an ether-like fluid serving as his life force.

With the appearance of Haller's doctrine of irritability as a specific property of living tissues, future systematists were no longer compelled to turn to metaphysics for a definition of their "life-force." William Cullen (1710-1790), the father of modern solidism, was the first to incorporate this theory in a system. Nerves were the chief agents of his solidism. His life-giving element was an undefined dynamic something (different from Hoffman's "ether" and Stahl's "soul") which he called nerve force.

Cullen's student, John Brown (1735-1788), took sharp issue with Cullen on the question of Hallerian irritability. Out of this argument arose the Brunonian theory, which was little more than Cullen's solidism combined with Asclepiadean Methodism.

The Brunonian theory, although not accepted to any degree in England, achieved widespread popularity in Germany, Italy, and America. Joseph Frank (1774-1841), a professor first at Pavia, then at Vienna, embraced the Brunonian system and with some modifications transplanted it to Vienna. Röschlaub (1768-1835) modified Brown's system into the theory of excitement (*Erregungstheorie*) which, for a time, was extremely popular in Germany. In America the chief advocate of Brunonianism was Benjamin Rush (1745-1813) who was followed by a considerable school. It was probably in Italy, however, that Brown had the most followers, under the leadership of Rasori and his successor, Tommasini.

The popularity of the Brunonian theory was due to its simplicity, totality, and logic. It mattered little whether or not it was shown to be true by experimentation. Its logic and totality were taken as sufficient, for Eighteenth Century philosophy had fostered a love of abstract speculation, and some sort of all-inclusive system was believed indispensable in every field of knowledge.

It is quite evident from the foregoing that Rasori's theory of *controstimolo* was far from original. Rasori's reasoning was no less deductive than Brown's, and he, as Brown before him, seemed more interested in being logical than in being right. Had this been all he offered medical posterity, he would have richly deserved the neglect he has received. But is it correct to classify categorically Rasori as a Brunonian? There seem to be many factors weakening this contention.

Among these factors is Rasori's interest in pathological anatomy. This interest was completely lacking in Brown, and quite understandably so, for the Brunonian theory was complete within itself. No need was felt for further elucidation of either the causes or effects of disease, because Brown had evolved a logical explanation for all this with his deductive philosophy. This would naturally make the study of pathological anatomy, at best, irrelevant. Therefore Rasori's interest in pathological anatomy indicates that he did not consider Brunonianism an adequate causal explanation of disease, and that he believed that only scientific investigations could determine causal factors. Thus we are not surprised to find that this "Brunonian" was an excellent anatomist, a keen observer, and an able scientist when he subjected his observations to unprejudiced reasoning.

One factor, however, stands out more prominently than all others against this classification of Rasori as a systematist. Systematists were often forced to include many sophistries in their explanations in order to preserve their systems. This, Rasori did not do. When faced with the fact that malaria did not fit into his system, he accepted the fact as it was, without trying to make it conform with his theory of diathesis. He approached the problem completely unprejudiced, and was able, somehow, to divine the correct answer 64 years before it was definitely demonstrated. His position on malaria and his acceptance of the germ theory of disease make one wonder whether in his later years the theory of *controstimolo* was anything more than a faint remembrance of his medical past. So it must have seemed to his faithful disciple Del Chiappa who objected strongly to the master's neglect of the diatheses in the *Teoria della flogosi*. Surely this does not seem typical of a supposedly uncompromising systematist. He might perhaps be accused of being misguided during his medical youth, but to typify him as a systematist only one step removed from Brown, would seem, in the light of what has been just pointed out, a gross inaccuracy.

A more appropriate term of comparison for him would be Broussais. Like Rasori, Broussais gave vent to anti-Hippocratic feelings, most likely inspired by Rasori's own anti-Hippocratic pronouncements in the lecture

of 1798. Like Rasori, Broussais was "Brunonian" in many respects (despite his vehement denial), but a Brunonian who realized the importance of pathological anatomy, and sought to explain disease on the basis of localized lesions. Once he had made this contribution, however, Broussais fell again into the evils of "essentialism" with his "gastroenteritis," so that when faced, as Rasori was, with the problem of malaria, he was pushed instead to the absurd sophism of "periodic gastroenteritis." Despite this, posterity was much kinder to Broussais than to Rasori. Broussais, with his concept of "lesions" and "localism" was the starting force of the Paris Clinical School, which was the precursor of modern scientific medicine.[26] True, he contributed little to this development directly, but his medical progeny, for which he is chiefly remembered, were certainly illustrious.

This raises a question in our minds. With such progressive ideas on pathological anatomy, on the germ theory of disease, and his surprisingly accurate concept of malaria, why didn't Rasori establish, or at least attempt to establish, a school of medicine based on these new principles? An answer to this question can possibly be found in certain important events in his life, and in the political state of Italy in the post-Napoleonic period.

Rasori's ideas on malaria and his support of the germ theory were not known until 1846, when Bassi published his pamphlet on pellagra. Why didn't Rasori express these ideas publicly during his lifetime? We do not know, but we may surmise that his experience of the Austrian prisons determined him to keep out of controversies and seek safety in obscurity. Prior to his imprisonment, he had been a firebrand who enjoyed heated polemics. He had been very active in politics and quite outspoken in his liberalism. But, after his release from prison, he was greatly chastened and subdued. He joined the *Il conciliatore* group when it was formed (1818), but withdrew from it shortly after, probably as soon as he realized the political leanings of his associates; and the persecution suffered by the other writers of *Il conciliatore* in 1821 may have confirmed him in his cautious line of conduct. A person in this frame of mind would naturally be averse to advance publicly radical ideas in any field. And radical those ideas must have appeared even to some of his faithful disciples, when one of them, Del Chiappa, refused to do Rasori the "injustice" of discussing his "erroneous" theory on malaria, feeling that every great man should be allowed some weakness.[27] A cautious Rasori therefore could only keep

[26] E. Ackerknecht, "Broussais or a Forgotten Medical Revolution," in *Bulletin of the History of Medicine*, vol. 27 (1953), pp. 320-321.
[27] F. Marimo, *op. cit.*, p. 163.

his ideas to himself, or discuss them privately with kindred spirits like Bassi.

Even if Rasori had retained the qualities of leadership and the willingness to fight for his convictions, it would have been impossible for him to establish a new trend in medicine. With the return of the Austrians in Lombardy in 1814 Rasori lost his professorship and with it the opportunity to influence succeeding generations of doctors. He no longer had students to whom he could pass on his interest in pathological anatomy, and who might conceivably have established a medical tradition similar to the Paris Clinical School. This was not possible for Rasori or for any other Italian who had been prominently connected with the Napoleonic regime since they would be understandably viewed with suspicion by the Austrians for that very reason. A bright scientific light could have served not only as a nidus for an Italian Clinical School, but also as a nidus for future political insurrections dedicated to political independence. To ward off this danger the Austrians maintained the strictest control over education, which caused a great deterioration in the quality of teaching in all the universities of the Lombard-Venetian Kingdom. Giuseppe Mazzini, in a pamphlet entitled *Italy, Austria, and the Pope* (1845) observed:

> The instruction given is upon the themes sent from Vienna; a maxim, an expression, a word deviating in the least from the prescribed doctrines involves the loss of the Chair, prison, persecution of all kinds. Hartman is the text prescribed to the fellow countrymen of Rasori, Tommasini, and Bufalini. The organization of the Faculties is left incomplete and in disorder. In the medical faculty, Anatomy, and Natural History are entirely taught in nine months. There are no theoretical principles for surgery; the practical part only is taught. Pathology, midwifery, veterinary medicine, materia medica, are jumbled into one and the same year, the third; there is no Chair for pathological and comparative anatomy; the same professor being engaged to give surgery lectures one year and to teach the operating practice the other, the student often finds himself put to one of the most difficult operations, before having tried his hand at the most simple one, before he is even acquainted with the first elements of Surgery. The Chairs in spite of the pretended competitorship are in general the price of some secret service promised or performed.[28]

This was the tragedy of Rasori. There was no great clinical school in Italy that could trace its ancestry to his interest in pathological anatomy. Therefore Rasori the pathological anatomist was forgotten, and only Rasori the Brunonian was remembered—his evil living after him, his good, being interred with his bones.

[28] G. Mazzini, "Italy, Austria, and the Pope," *Scritti editi e inediti*, Imola, Galeati, 1905-1943, vol. 31, pp. 246-248.

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MEDICINE AND GRAECO-ARABIC ALCHEMY *

OWSEI TEMKIN

In the wake of a medieval tradition, the medical profession of England, some 150 years ago, was divided into three sections. There was the learned doctor of medicine whose task was to treat the patient as a whole by regulating his diet and manner of life, prescribing medicines and advising operation wherever necessary. The doctor of medicine had a counterpart in the surgeon and a competitor in the apothecary. The surgeon was wont to look upon disease with the eyes of an anatomist, locating it in some part and organ where, if possible, it would be accessible to ligatures, dressings, and the knife.[1] The apothecary, on the other hand, originally confined to the selling of drugs and making up of prescriptions, had taken it upon himself to prescribe his products. His point of view, naturally, was to have a proper label for each remedy stating for what complaint it should be used and, conversely, to classify symptoms and diseases so that they would correspond to the list of drugs.[2] Yet as the surgeon had his science of anatomy, the apothecary also had his favored science, viz. chemistry, that taught him the material properties by which the drug affected the material processes in the body.

Since treatment by drugs and chemistry seem to go together, it is not surprising to find in antiquity already, examples of chemical procedures connected with the preparation of drugs. The fifth book of Dioscurides' "Materia Medica" includes the medicinal use of mineral substances, and here one of the earliest instances of distillation (or condensation) is to be found in the chapter on mercury.[3] True, Dioscurides like Galen [4] after him considered mercury a poison; nevertheless, this and other passages attest the acquaintance of the ancient apothecary with chemical techniques. Some four hundred years after Galen, Paulus of Aegina stated that some

* Read at the twenty-seventh annual meeting of the American Association of the History of Medicine, New Haven, Connecticut, May 7, 1954. (Revised).

[1] See Temkin (4), p. 254.

[2] Nicholas Culpeper's (1616-1654) English editions of the Pharmacopoeia Londinensis illustrate this trend which, however, goes back to the old herbals and the medieval Mesue.

[3] Dioscurides V, 95; Wellman, vol. 3, p. 66. See also von Lippmann (1), p. 71, and von Lippmann (2), vol. 1, p. 10; Cohen and Drabkin, p. 353.

[4] Galen, De simplicium medicamentorum temperamentis ac facultatibus V, 19 and X, 32; ed. Kühn, vols. 11, p. 765, and 12, p. 237.

people burned mercury and used the ashes together with other drugs internally for colic and ileus.[5]

We should of course like to know more about these people who recommended mercuric oxide (if this is what they obtained). Were they medical men like Dioscurides and Galen, or were they alchemists, or possibly alchemists practising medicine? This question opens a wider one. What was the relationship between alchemy and medicine, and when did the two meet? This is a problem which is not without importance, since Western chemistry, as we understand it, is supposed to have developed from alchemy. Dr. Multhauf, in an article just published, has thrown doubt on the assumption that medical chemistry developed from the new orientation given to alchemy by Paracelsus.[6] This makes it all the more interesting to go back to the beginnings of medieval alchemy and its relation to medicine. In the following I wish to make a few tentative observations on this intriguing yet very complex subject.

At this point, some orientating remarks may be helpful. There is a collection of Greek alchemical writings which date from about 300 to 700 A. D.[7] They are attributed to various authors, some of them historical persons, others legendary. Unfortunately, most of the Greek texts are still in an utterly confused state.[8] They deal with the sacred art of transmuting lower substances into silver and gold, the avowed aim of alchemy. But as to method, they differ widely from technical recipes to symbolic works of a gnostic character. This alchemy had a Syriac successor, just as Greek science and medicine found their way from about 500 A. D. on into the new garb of Syriac.[9] Regarding medicine, the transmission was due to Nestorian Christians who had their main seat of learning in Gundē Shāpūr in the province of Khūzistān in southwest Persia, and to Jacobite Christians further west. It is likely that alchemy too was cultivated in the same centers. Between about 750 and 950, medicine underwent a second transformation into Arabic, largely on the basis of preexisting Syriac translations. And alchemy too not only found a home in Arabic civilization but a much greater flowering than ever before. Rhazes

[5] Paulus Aegineta, vol. 3, p. 385; also vol. 2, p. 238 f. for Adams' valuable comments on the use of mercury among the Arabs.

[6] Multhauf, pp. 102 ff., 120 ff., and 125 f.

[7] Berthelot and Ruelle, and Ideler give the bulk of the ancient alchemistic material though not always in the most recent edition available. For further details see von Lippmann (2) and Sarton.

[8] Rehm discusses the status in his review of volumes 4-8 of the *Catalogue des manuscrits alchimiques grecs.*

[9] The bulk of the Syriac alchemistic literature is to be found in vol. 2 of Berthelot (2).

and Jābir, both writing about 900 A. D., are the two greatest names in the history of Arabic alchemy and, perhaps, of alchemy in the tradition of Graeco-medieval science altogether. We owe to the late Paul Kraus a chronology of the books of the Jābir Collection according to which the earliest only precede the writings of Rhazes.[10] Kraus has shown that Jābir's work was inspired by the religious sect of the Ismā'īlīya and that the alleged relation to Ja'far of the eighth century is a product of Ismā'īlīya propaganda.[11] Moreover, Kraus has made available a selection from the Jābir works [12] which extends our knowledge beyond the treatises previously published.[13] On the other hand, Ruska's many publications,[14] notably his German translation of one of Rhazes' main alchemical works, the *Sirr al- asrār*, the " Secret of Secrets," [15] allows a real insight into the labors of the most chemically minded of the alchemists.

We are thus able to compare late Greek alchemy with Arabic alchemy of about 900. This comparison seems to indicate a difference in the relationship to medicine. Rhazes was one of the greatest physicians of all time, famous, above all, as a clinician. The Jābir collection also contains medical works; [16] whoever the man behind this name may have been, there is little doubt that he was a physician.[17] But if we turn to late Greek lists of alleged or real alchemists, we do not find a single outstanding medical name among them.[18] Thus Stephanus is the author of an alchemistic

[10] Kraus (4), vol. 1, p. lx f.

[11] Kraus (1) and (4), vol. 1, p. xlv ff. Cf. also Taylor (5), p. 78 ff.

[12] Kraus (3).

[13] Mainly the third volume of Berthelot's *Chimie au moyen âge*, and *The Arabic Works of Jābir ibn Ḥayyān*, edited with translation into English and critical notes by Eric John Holmyard, vol. 1, part 1 (Arabic text), Paris, 1928 (I quote the title after Kraus (4), vol. 1, p. 199, since Holmyard's edition was not available to me). I am not including in this study the Latin works of Geber since his relationship to Jābir is not yet sufficiently clear.

[14] A chronological list of Ruska's publications up to early 1937 was published in *Abhandlungen zur Geschichte der Medizin und der Naturwissenschaften*, Heft 19, Berlin, Ebeling, 1937.

[15] Ruska (5). The English reader will find information on this work in Stapleton and Ḥusain, and on Rhazes and alchemy in general in the articles by Heym and Partington.

[16] See Kraus (4), vol. 1, pp. 155-160. Jābir's " Book on Poisons " (Kraus, *ibid.*, p. 156, no. 2145) seems the only medical work extant; its content has been analyzed by Ruska (3).

[17] Kraus (2), p. 19.

[18] Berthelot and Ruelle give two lists: Vol. 1, pp. 110-111, from Cod. Marcianus 299 (Venice) bears the title, " Names of the philosophers of the divine science and art." Vol. 2, p. 25 f. (translated vol. 3, 26 f.), from Paris manuscripts, starts out, " Know, my son, the names of the workers too." Berthelot (1), p. 128 ff., has commented on these as well as a shorter list. For a more recent attempt at identification of the names see Taylor (1), p. 113 ff.

work, in part addressed to the Byzantine emperor Heraclius (610-641 A. D.),[19] but his identity with a physician Stephanus of about the same time, who commented on works of Hippocrates and Galen, is quite uncertain.[20] It has been widely assumed that the name of Sergius mentioned in the list refers to Sergius of Rēsh'ainā (died 536 A. D.) who visited Alexandria and translated Greek medical and philosophical works into Syriac.[21] The oriental tradition ascribes alchemistic works to him [22] and he is quoted by both Rhazes [23] and Jābir.[24] However, the genuineness of these works has been doubted [25] and if the doubts were to prove justified, there would be reason to assume that the Syrians and Arabs when coming across the name of one Sergius, identified him with the translator who was so well known to them. At any rate, the lists of authors make it clear that the Greek alchemists considered the rise of their art as something outside of medicine. Even the association with Aristotelian philosophy, so powerful in other branches of Greek science and medicine, seems to have come late. In one list of names, Plato and Aristotle do not figure at all,[26] and a philosophical theory in contrast to purely gnostic-hermetic tendencies becomes discernible only with Olympiodorus towards the end of the sixth century.[27]

An eminent Alexandrian commentator of Plato and Aristotle, Olympiodorus seems to have made a place for alchemy among philosophical studies.[28] Now we know that academic medicine in Alexandria was in contact with Neoplatonic philosophy.[29] It would, therefore, not be surprising to find a closer relationship between physicians and alchemists

[19] The work was edited by Ideler, vol. 2, pp. 199-253. A new edition with English translation was begun by Taylor (4).

[20] On this question as well as for Philaretus see Temkin (1), pp. 51-74. Taylor (1), p. 114, points out that "a very late list" refers [among other names] to Dioscurides as an alchemist.

[21] Berthelot (1), p. 205, who, moreover, identifies him with the addressee of the work "On the Divine Water" by "the Christian" (Berthelot and Ruelle, vol. 2, p. 399, and vol. 3, p. 386 respectively).

[22] Baumstark, p. 172.

[23] A quotation occurring in Rhazes' "Book of Evidences" is cited by Stapleton and Azo, pp. 72 and 89.

[24] Kraus (4), vol. 2, p. 45, particularly footnote 2.

[25] von Lippmann (2), vol. 1, p. 355, Baumstark, and Kraus (4), *locis citatis.*

[26] Berthelot and Ruelle, vol. 1, pp. 110-111; cf. above, footnote 18.

[27] See Kraus (4), vol. 2, p. 34, who accepts the identification of the alchemist with the Neoplatonic commentator as suggested by Tannery. The same identification has been made by Reitzenstein as well as Hammer Jensen, p. 125 ff. For other opinions see Bentler.

[28] Hammer Jensen, p. 128.

[29] Temkin (2), p. 423 f.

in the generation contemporary with and following upon Olympiodorus, i. e. approximately the generation of Paulus of Aegina. It is indeed worth noting that a Byzantine alchemistic poem, written by one Theophrastus after 600 A. D.,[30] appears to be one of the earliest testimonies for a linking of physician and alchemist. Theophrastus starts out with a reference to men who know the nature of all things.[31] He accounts briefly for their skill in the various sciences beginning with astrology and then proceeds to medicine. In Browne's translation the passage reads:[32]

> Yet more than this, the causes we reveal
> Of each affliction in the body's frame;
> Experimentally our school explores
> The science, art and ends of medicine,
> With such success that our prognosis shows
> What sicknesses are destined to appear
> And what is best to cure or ward them off;
> Its findings also lead us to foretell
> An end of life from sickness far from home.

After medicine, Theophrastus discusses the investigation of vegetables, minerals, and animals, and since he speaks about their good and evil, this section may be interpreted as having some pharmacological meaning. Subsequently he turns to the detractors of alchemy and to alchemy itself. Reitzenstein suggested that the author used the name of the great philosopher and scientist Theophrastus in justification of alchemy.[33] The

[30] An English translation has been prepared by Browne (1), based on Ideler's Greek text; a more recent edition was published by Goldschmidt subsequently to Reitzenstein's work. Reitzenstein (pp. 36 and 28) and Goldschmidt (pp. 13 f. and 21) are convinced that the real author was Heliodorus, who also wrote under the assumed names of Theophrastus, Archelaus, and Hierotheus, and who flourished around 716 A. D. See also Browne (2).

[31] Browne (1), p. 194; Goldschmidt, p. 34, 1 ff.

[32] Browne (1), p. 195; Goldschmidt, p. 35, 33-42:

> καὶ οὐ μόνον ταῦτ' ἀλλὰ καὶ παθημάτων
> τὰς αἰτίας φαινοντές ἐν τῷ σώματι
> ἰατρικῆς τέχνης τε καὶ ἐπιστήμης
> ἐμπειρίαν καὶ πρᾶξιν ἐντέχνου τέλους
> ὁρίζομεν μύησιν, ὥστε καὶ λέγειν
> προγνωστικῶς μέλλοντα γίνεσθαι πάθη
> καὶ ἔκβασιν νόσου τε καὶ ἄκος φέρειν
> καὶ μηνύειν ἔκδημον ἐκ νοσήματος
> βίου τελευτήν, ὡς προγινώσκειν σαφῶς
> καὶ τοὺς πόνους τῇ πείρᾳ καὶ παύειν νόσον.

[33] Reitzenstein, p. 31 ff.

medical passage would then be merely an allusion to Theophrastus' physiological and pathological interests.[34] Nevertheless, the poem indicates that around 700 it was no longer considered strange to have the same person cultivate medicine as well as alchemy.[35] Such a lowering of the barriers, if it took place, was of academic rather than practical interest, since at this late period Greek alchemy had become a literary art, experimental work having all but ceased. At any rate, we know too little of Greek medicine and of Greek alchemy around the time of the Arabic conquest of Alexandria to arrive at pertinent conclusions. In both fields the events in the East were beginning to overshadow those in the Byzantine West.

If we now turn to the literary tradition of the East, both Syriac and Arabic, we find a definite tendency to establish an historical connection between physicians and alchemists. As mentioned before, Sergius of Rēsh'ainā, rightly or wrongly, was credited with the authorship of an alchemistic work. Zosimus, the first authentic Greek alchemical writer, was now supposed to have written a pharmacological treatise based on Dioscurides and Galen.[36] Rhazes was cited as stating that philosophers like " Pythagoras, Democritus, Plato, Aristotle and, finally, Galen engaged in the art."[37] And this legend found support in the Jābir collection where Galen was said to have studied alchemy before turning to philosophy,[38] and where finally even Hippocrates was included.[39] In other words, the Orient, looking backwards, incorporated the names of famous physicians in its history of alchemy because in the Orient a personal union between alchemist and medical man had been established.[40]

[34] *Ibid.*

[35] I do not mean to say, of course, that before about 600 A. D. physicians could not be found among alchemists. But such a union, where it existed, probably was a personal affair rather than a planned merger of professional interests.

[36] Baumstark, p. 173.

[37] An-Nadīm, p. 352; cf. Berthelot (2), vol. 3, p. 27.

[38] Kraus (4), vol. 2, p. 326, footnote 1. Because of their great similarity in Arabic, the names of Galen (Jālīnūs) and Apollonius of Tyana (Balīnūs) have undoubtedly often been confused, especially since the latter played an important role in Arabic alchemy (see Kraus, *ibid.*, p. 270 ff.). However, this does not permit a more or less arbitrary replacement of Galen's name by that of Apollonius. Since An-Nadīm mentions Galen in connection with Rhazes (see above and footnote 37), an argument ex silentio on the part of the *Fihrist*, as Ruska (4), p. 289, claims, does not exist.

[39] Kraus (4), p. 42, footnote 5.

[40] In spite of the fact that in the Latin West, too, Plato and Galen appear as alchemical authors (see Thorndike, vol. 2, p. 782 f., and Singer), it is not very likely that the Orient took over a late Greek tradition. Yet it is not unlikely that to the Syrians and Arabs the occasional reference to the name of a physician (e. g. Berthelot

Ruska stated long ago that it was among the pharmacists and physicians that alchemy was cultivated in the East.[41] From what we have seen it would indeed be hard to deny this statement. But the mere external connection of two arts in the same person is of relatively little interest as long as we cannot establish an inner connection. Searching for such an inner connection we shall ask in how far alchemy depended on medicine and in how far medicine became dependent upon alchemy. Even the Greek alchemistic texts show some dependence upon medicine. This is to be expected since, in the preparation of drugs, techniques were used similar to those of other crafts from which the alchemists borrowed.[42] For instance, the practitioner is advised to grind and cook a mixture "in the medical manner."[43] The alchemistic philosopher is warned against thoughtless polypragmaty; instead he should take an example of the physicians who ponder the qualities of the various drugs they use.[44] Moreover, alchemistic procedures are compared to organic processes, e. g. the growth of the embryo, the nourishment of the body, and so on.[45] There is even a certain community of terms. The Greek term for tincture, *baphē*, i. e. dyeing, is also used for infection; the alchemist uses *pharmaca*, "drugs," for his transmutations just as the physician does for the treatment of diseases, and the alchemistic manuscripts contain symbols for medicinal substances.[46]

Among the Arabic alchemists, more particularly the works ascribed

and Ruelle, vol. 2, p. 272, Galen is quoted by name together with the opening passage from his "De locis affectis") suggested a closer relation with medicine than the Greek texts warranted. Kraus (4), vol. 1, p. 54, note 4, mentions the possibility that the oriental alchemists were inspired by the ancient tradition of medical history.

[41] Ruska (1) and (5), pp. 6 f. and 71.

[42] This point is strongly emphasized by Hammer Jensen, pp. 42 f., 56, 89, 92 ff. and *passim*. She may have gone too far when she says, p. 93: ". . . ja man fragt sich hier wie öfters, ob der erste Alchymist nicht ein griechisch gebildeter Arzt war."

[43] Berthelot and Ruelle, vol. 2, p. 55 f. (vol. 3, p. 59 f.). In addition to the passages quoted in the preceding footnote, Hammer Jensen, pp. 51, 59, 62, 66 f., 87, has collected so many parallels between medical and alchemistic practices that I can limit myself to a few examples.

[44] Berthelot and Ruelle, vol. 2, pp. 47 and 103 (vol. 3, pp. 50 and 112); see also vol. 2, p. 49 (vol. 3, p. 53) and Hammer Jensen, p. 89.

[45] E. g., Berthelot and Ruelle, vol. 2, pp. 170 and 203 (vol. 3, pp. 169, 198). For the biological comparisons cf. Hammer Jensen, p. 16 f.

[46] Berthelot and Ruelle, vol. 1, p. 92 ff. *Catalogue*, vol. 8, p. 1 ff.; see also Taylor (3). There exist also comparisons with the humors of the body (e. g. Stephanus in Ideler, vol. 2, p. 200) and even with diseases and surgical treatment (Berthelot and Ruelle, vol. 2, p. 233, i. e. vol. 3, p. 226). Not only Stephanus (as Hammer Jensen, p. 154, points out) but other Greek alchemistic authors too show an acquaintance with medical theory. Yet this acquaintance does not go beyond what might be expected of an educated person.

to Jābir, comparisons between the two arts are combined with the integration of long biologico-medical chapters in alchemical texts. The experienced alchemist is compared to the experienced physician and the latter's knowledge of the nature of stones, plants, and animals should be utilized.[47] The unity of the elixir is likened to that of the theriac, and the elixir is said to act in the manner of a poison.[48] The so-called " Little Book of the Balances " resembles a magico-medical work with rather bad physiology; only its very end has a possible connection with alchemy.[49] And the " Book of Concentration " is really a metaphysico-physiological treatise.[50]

It is not necessary to heap the examples in order to show that the Arabic alchemist is expected to possess—or to acquire—some knowledge of medicine. This seems particularly true of the Jābir texts in which, as Kraus has shown, Galenic pharmacological theory is expanded into an all-embracing theory of numerical relations.[51] According to Galen and other Greek physicians, drugs acted by their respective qualities of cold, warm, dry, and wet. However, simples were not only distinguished by having these qualities in different proportions. Galen distinguished four degrees of action of any one quality, and there were gradations within each degree. This theory was accepted as well as refined by Arabic physicians. It is referred to in the *Paradise of Wisdom* of Aṭ-Ṭabarī, a book written in 850, and more important, the great Arabic philosopher-physician Al-Kindī wrote a little treatise in which he extended the theory to compound remedies. Al-Kindī died around 870; his treatise was translated into Latin and became a standard work of medieval posology.[52] We need not be concerned here about the various influences, Neo-Pythagorean above all, that made Jābir elaborate these ideas into a fantastic system of numerical relationships between things. Suffice it to state that this system has a medical background and plays an essential role in the composition of the elixir with which transmutation into gold is to be effected.

So far, then, we may say that in the East alchemy passed into the hands

[47] Berthelot (2), vol. 3, pp. 164 and 166.

[48] *Ibid.*, pp. 171 and 174 f.

[49] *Ibid.*, pp. 139-162.

[50] *Ibid.*, pp. 191-206. See also pp. 200-203 for an outline of ancient theories of digestion and assimilation.

[51] Kraus (4), vol. 2, p. 189 ff., where the pertinent passages in Galen are quoted and references given to Aṭ-Ṭabarī, Al-Kindī, and others.

[52] The translation has the title " De medicinarum compositarum gradibus investigandis libellus " and is found in various editions of the *Opera* of Joannes Mesue Damascenus, e. g. Venice, Junta, 1623, vol. 2, fol. 269 verso-273 verso. An analysis of the work on the basis of an Arabic text (Ms. Munic 838, fol. 28-32) has been published by Siggel.

of medical men and increasingly utilized the latter's knowledge. But our main interest focuses upon the question as to whether the reverse also was true, whether medicine profited by alchemy.

We had best start with an example, the preparation of the famous "oleum benedictum" as we find it in the Latin text of Rhazes' "Antidotarium." [53]

Take old oil and fresh red bricks not touched by water. Break them up into small pieces approximately to the weight of one drachm, and heat them in fire until they become very red. Then dip each piece singly in the oil, grind them and fill with them glazed vessels which can stand the force of fire. Afterwards close with clay [54] and hang them up in a furnace just as a vessel with rose oil is suspended, there must be nothing between it and the fire. When the clay has dried, gently and slowly light a fire under the vessels, and the more heated they become the greater the fire should be, until water flows from the vessels.[55] Then you should begin to strengthen the fire until you see a very red oil flowing. And take heed lest the oil touch the fire, for it should not be extinguished afterwards, rather the vessels should continue to be close to the fire until the oil ceases to flow. Then allow the fire to die until the furnace becomes cool. Afterwards remove the sediment from the vessels and add other bricks, if the vessels are intact; otherwise take other vessels and do this until you have enough oil. Put it in a vessel with a narrow opening and close with wax. This oil belongs to the secrets of the physicians and has the same use as the oleum balsami. But it is more subtle, warmer and of greater usefulness in cold diseases . . .

At first sight any doubts as to considering this an application of alchemistic technique to the preparation of a drug would appear unreason-

[53] Rhazes (2), p. 557. The oil is here called "oleum benedictum"; it was also known as "oleum laterinum" (see von Lippmann (2), vol. 1, pp. 480 and 485) or "oleum philosophorum." The "Grabadin" of Joannes Mesue, *Opera*, vol. 1, fol. 193 verso, gives a whole list of names: "Oleum philosophorum (quod etiam oleum sapientiae, et perfecti magisterii, et benedictum, et divinum, et sanctum quidam vocarunt) . . ." The occurrence of this recipe in the works of Rhazes does, of course, not prove him the inventor. "Destezberigi medicamentum acutum et corrosivum" which is found in the seventh book of Rhazes (2) "Ad regem Mansorem," p. 166, shows that he used chemically produced remedies invented by earlier authors. The identity of this "Destezberig" is unknown to me; the recipe reads: "Take ½ pound each of ground yellow arsenic, chal (?), lime, ziniar (copper acetate); one pound of mercury, ¼ pound of sal amoniac. Let all this be ground with aqua prima until the mercury is extinct, let it then be dried and ground again. Afterwards, let it be treated over a fire in the instrument which the alchemists call alutel until there is ascent and sublimation. Take away what has become sublimated and keep it in a glass vessel." This medicament for external use is supposed to eliminate superfluous flesh.

[54] "Lutum sapientiae," i. e. the clay used by alchemists.

[55] Since the process involves distillation, the description seems incomplete. We have to imagine the vessel connected with a recipient for the product. The description by Mesue, *loc. cit.* (above, footnote 53), is very detailed on this point.

able. It is obvious that Rhazes—always provided that we really are dealing with his own words—is referring to apparatus used by alchemists. But that does not prove that such recipes necessarily represented a direct loan. Karl Garbers, in 1948, published the " Book of the Chemistry of Perfumes and Distillations " by the same Al-Kindī we mentioned before.[56] It is a very practical work dealing with the preparation of cosmetics, partly with and partly without distillation and similar procedures. Where the latter are involved, the apparatuses are described in detail as they are in alchemistic works. Moreover, Al-Kindī, without any sign of embarrassment, teaches the falsification of perfumes; for instance, in a recipe for musk he advises pounding together a number of vegetable substances with a drop of jasmin oil and continues: " Then put it [i. e. the mixture] on a new thick piece of cotton and smooth it well until the grease of the oil is transferred into the cotton piece. It will come out well. Take two parts of it, mix carefully with one part of musk and it will sell as if it were a homogeneous, good and excellent substance—if Allah will." [57] This is remindful of the recipes of workers in gold and precious stones who taught how to make things look like real gold and jewels. Moreover, Al-Kindī reputedly was an enemy of alchemy proper,[58] and whether this is true or not, his book shows that a chemical industry existed by about 850, an industry that utilized apparatuses and methods similar to those of the alchemists and probably included drugs as well, especially those applied externally.[59] The relatively few chemically prepared medicaments I was able to find in Rhazes' work relate chiefly to external use. The direct influence of alchemy is thus hard to evaluate, the indirect, however, seems certain.[60]

[56] See above, p. 141.

[57] Al-Kindī, p. 31 of Garbers' German translation, and p. 1 of the Arabic text.

[58] See von Lippmann (*2*), vol. 1, p. 399.

[59] The relationship of alchemy to chemical technology, both Greek and Oriental, is as yet a matter of dispute. Chemical knowledge and alchemical aim should, however, not be confused. Rhazes was an alchemist, in spite of his clear grasp of chemical methods. On the other hand, Al-Kindī's familiarity with such procedures as were used by alchemists does not by itself make him an alchemist. The article by Pigulevskaya (a French summary of which appeared in *Isis*, 1937, vol. 27, p. 159 f.) is quite inconclusive since it does not differentiate between techniques developed by alchemists and traditional pharmaceutical procedures.

[60] I have not been able to find many recipes in the Latin *Continens* or the *Opera exquisitiora* of Rhazes involving use of distillation or similar chemical methods. Ruska (5), p. 6 f., gives an analysis (after Ibn Abī Uṣaibi'a) of some chapters from Rhazes' *Kitāb al-jāmi' al-kabīr* that would indicate a systematic application of chemical methods in medicine (Ranking on p. 256 gives the Arabic titles and Latin translation of the 12 chapters of this work). Ruska claims the *Kitāb al-jāmi'* to be different from the *al-Ḥāwī* (Continens), a claim that has not yet been proved.

That the possible borrowings from alchemy should mainly involve external remedies is understandable when we consider that most alchemists largely relied upon mineral substances. The internal use of minerals, particularly metallic drugs, was limited although known since ancient days. The use of such metals as mercury (to revert to our initial example) seems to have been appreciated in the East more than it had been in the West. When syphilis first appeared, or was first diagnosed in Europe, the treatment of the disease by mercury followed almost immediately.[61] It has long been assumed that the recommendation by Arabic authorities of mercury against skin diseases was responsible for its early administration against syphilis. Aṭ-Ṭabarī prescribed it for mice, lice, and skin diseases,[62] and he may not have been the first to do so. The *Continens* of Rhazes has a passage that reads: "Chuz says that it is good for scabies and evil sores and that its powder kills mice."[63] "Chuz" is commonly interpreted as referring to the Hussi, the people of Khūzistān, the province in which Gundē Shāpūr was located, so that the reference probably is to the physicians of that famous early center.[63a] This narrows the gap to Paulus of Aegina to less than 250 years.

I have stressed the external use of chemically prepared drugs and the medical limitations imposed upon their internal use because there also existed a different alchemy with different medical scope. I have in mind the internal use of the elixir as described in the Jābir writings. Again we had best start out with an example, this time a case history introduced by Jābir's claim of having saved more than a thousand persons by means of the elixir.[64]

One day, after my reputation in these sciences and as a servant of my master[65] was already established, I was with Yaḥyā Ibn Khālid.[66] He had a noble slave-

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[61] Sudhoff, pp. 55 and 117 ff.

[62] Aṭ-Ṭabarī, p. 408. The reading of zaibaq instead of zanbaq is due to Meyerhof (1), p. 36. The Arabic word *fa'r* (or *fār*) may mean mouse or rat.

[63] Rhazes (1), Tractatus primus libri ultimi, c. 75, fol. viii recto: "Dixit Chuz: quod confert scabiei et ulceribus malis et pulvis ipsius interficit mures." The question arises as to whether Indian influence might be responsible for the wider use of mercury. The chronology of the early history of Indian chemistry and medicine seems so uncertain that no valid conclusions can be drawn. See von Lippmann (2), vol. 3, p. 25 f.

[63a] See Meyerhof (2), p. 20.

[64] A tentative German translation of the following story (together with other examples of the medicinal use of the elixir by Jābir) is given by Kraus (2), pp. 21-22. The Arabic text appears in Kraus (3), p. 303 f., and an improved French translation in Kraus (4), vol. 1, p. xxxviii, note 5; the English translation is based on the text and Kraus' two versions.

[65] A reference to Jābir's alleged teacher, Ja'far aṣ-Ṣādiq.

[66] The famous Barmakide vizier who lived about 738-805.

girl; no one else owned her equal in beauty, perfection, culture, intelligence, and skills. Because of an illness which she had, she had taken a purgative, which, however, affected her direly and she vomited so much that for a person like her one could expect no salvation or cure. The vomiting exhausted her, so that she had no longer any power to breathe or speak. A messenger went out to Yaḥyā who said to me: "My Master, what do you think about the situation?" I advised him to pour cold water over her, for I had not seen her; and besides, I knew of no cure for such poisoning.

But neither cold things nor hot were of any avail. For I had also ordered warming her abdomen with warm salt and covering her feet. However, when things became worse, he asked me to look at her. I saw her almost dead, her strength very much depleted. But I had a little of the elixir with me and of this I made her drink the amount of 2 grains in 3 oz. of pure oxymel. By God and by my Master, I had to cover my face before the maiden, for in less than half an hour her perfection was restored even to a higher degree than she had formerly possessed.

Thereupon Yaḥyā prostrated himself before me and kissed my feet. I said to him: "My brother, do not do so!" Then he asked me about the use of this medicine, and I told him: "Take what I have on me." However, he did not do so.

In this story two points deserve comment: First, the circumstance that an elixir is supposed to be capable of saving people from fatal sickness and, second, the miraculous character of the tale.

I do not know whether, in the Graeco-Arabic tradition, Jābir was the first to endow the elixir with medical potency, and it is impossible to tell whether the latter was entirely missing from the attributes of the philosopher's stone of the ancients. There is a Greek recipe for "the divine water" that heals every disease, makes blind eyes see, deaf ears hear, and the dumb speak clearly, that resuscitates the dead, makes the living die, elucidates obscure things, and obscures clear things, etc. The pretended authorship of Ostanes as well as the recipe itself leave it undecided whether all this is supposed to have any literal meaning or whether we are dealing with a purely symbolic text.[67] In particular, the disease may not be a physical ill, but the malady the alchemist was most eager to cure: poverty.

Regardless of whether the elixir had a Greek predecessor, we must not overlook the existence of other than Greek influences on Arabic alchemy. There are first of all the Syriac and Persian centers whose own

[67] Berthelot and Ruelle, vol. 2, p. 261 f. (vol. 3, p. 250). In the Greek text edition, the editors have the water cure *pān nosēma*, relegating *touto to nosēma* to a manuscript variant. But in the translation they use this variant and interpret "cette maladie" as poverty. See also Taylor (2), p. 43 ff. Taylor (5), p. 66, thinks that the main feature lacking in Greek alchemy "is that of the elixir or philosopher's stone." Kraus (4), vol. 2, p. 3, note 1: "L'emploi médical de l'élixir, si répandu dans les alchimies latine et indienne . . . semble inconnu des auteurs grecs."

share may have been far from negligible.[68] And while it is difficult to evaluate the possible contribution from India, Chinese influence apparently had reached Bagdad by the ninth century.[69] This is all the more important since Chinese alchemy had long aimed at rendering man immortal.[70]

Jābir's alchemy seems to foresee a place for medical action of the elixir, since it views transmutation itself as the cure of a malady. To quote Kraus: "Just as the physician cures the sick body by counter-balancing an excessive humor by a drug of opposite quality, so the alchemist will employ 'medicines' which are appropriate to cure the 'diseases' of the metals. And these 'medicines' are nothing but the elixirs. The true originality of the Jābirian alchemy lies in the theory of the elixir. Contrary to the Greek alchemists as well as the majority of Arabic alchemists, Jābir pretends that the elixir can be produced not only from mineral substances but also from vegetable and animal substances. And he gives preference to the elixirs produced from animal substances as a starting point, since they possess a much higher degree of organization than do the mineral and vegetable substances."[71] Moreover, Kraus points out that the reduction of metals into their alleged natures by means of distilled organic products, which forms the novelty of Jābir's alchemy as compared to that of the Greeks, coincided with the introduction of the sal ammoniac, ammonium chloride, as a new substance by the oriental alchemists. Jābir distinguished between the mineral drug and the one produced from such organic substances as hair and urine, the latter derivation playing a great role in his work.[72]

It seems to me that these characteristic features of Jābir's alchemy would well agree with his medical outlook. If the elixir was viewed as a remedy able to cure metals and men,[73] its preparation from organic substances, traditionally preferred for drugs, as against mineral ones, would be understandable.

Of course we should like to know whether the elixir was actually dispensed by Jābir and what its effects were. There is no reason to doubt

[68] Stapleton and Ḥusain have stressed the importance of Ḥarrān.

[69] Dubs, p. 84. Whether Chinese alchemy had originally influenced Greek alchemists, a possibility Dubs discusses on p. 81, does not concern us here.

[70] *Ibid.*, p. 69 ff.

[71] Kraus (4), vol. 2, p. 2 f.

[72] *Ibid.*, p. 41.

[73] Von Lippmann (2), vol. 1, p. 367; Diepgen, p. 10 f. In this connection the probable derivation of the Arabic *al-iksir* from the Greek *xerion*, powder for wounds [see Ruska (2)], is not without interest. Hopkins, p. 344 ff., denies altogether that the preparation of the (transmuting) elixir was peculiarly Islamic; he believes that it took the place of the Greek *Ios*.

that alchemist-physicians, perhaps even before Jābir, thought themselves in the possession of an elixir, gave it to sick people, and believed that they had effected a cure. But the work from which our case history is taken relates it to the sphere of the occult. It occurs in the *Kitāb al-khawāṣṣ*, the "Book of Properties." Rhazes also wrote a work with the same title.[74] By properties of things was understood the power of their specific nature which allowed them to act in a miraculous way. To quote Aṭ-Ṭabarī: things have "a property the cause of which is not known and the extent of which is grasped by experiments only,[75] for they are occult properties hidden in the things like the property of the loadstone with which iron is attracted . . ."[76] Plants, animals, and minerals played their roles in this peculiar mixture of folklore, experiment, and learned superstition. It had its literary founder in Bolos of Mendes, "the Democritean" who lived around 200 B. C.[77] In course of time, this occult lore invaded not only authors like Pliny but, to some extent, also professional medical practice and literature. Dioscurides has a good deal to say about the medicinal virtues of blood, urine, and faeces, and by the sixth century amulets appear in the works of such conservative authors as Alexander of Tralles, not to speak of their Latin contemporaries.

The Hippocratic tradition was against admitting the miraculous into medicine. Even in Hellenistic times it apparently remained strong enough to keep the leading physicians away from the gnostic beliefs that made alchemy a mystic religion.[78] But it yielded to the claim of alleged efficaciousness of what we call superstitious remedies.[79] Peculiarly enough, as far as I can see, the science of the occult properties and alchemy also preserved some independence from each other, in spite of strong personal and ideological ties between them.[80] Such separations, prevailing in Greek

[74] Kraus (4), vol. 2, p. 63.

[75] lā yudraku ghauruhā 'illā bittajāribi.

[76] Aṭ-Ṭabarī, p. 356, line 7 ff. I am indebted to Mr. W. Bishai and Mr. R. Hassow for corrections in my reading of this passage.

[77] On Bolos the Democritean and the literature of the "physica" see Festugière (2), p. 196 ff.

[78] The gnostic character of alchemy has been particularly emphasized by Hammer Jensen, and Festugière (2), p. 260, speaks of "l'alchimie religion mystique," in connection with the work of Zosimus. This poses the question in how far the Greek alchemistic writings echoed Oriental sources. Reitzenstein, p. 21, even believes that an Aramaic text was translated into Greek, mistakenly ascribed to Komarios, and incorporated into the Greek alchemistic collection.

[79] Edelstein, and Temkin (3), p. 21 ff.

[80] "Democritus," i. e. Bolos of Mendes, and the legendary sage Ostanes (on whom see Bidez and Cumont) figure as alchemists as well as miracle workers in the Greek tradition. The ideological tie is largely to be found in the hermetic background. Festugière (1) pp.

civilization, did not survive in the East where Hermetic philosophy, a common ground for everything occult and alchemy, became widely spread in various forms.[81] By 900 A. D., the elixir, a distinctly alchemistic product, had entered the list of occult remedies and a threefold union had thus been effected.

We shall now sum up by attempting to give a picture of the possible relationship between medicine and alchemy as developing between about 300 and 900. But we must preface this attempt by the warning that it cannot claim to be more than a working hypothesis. Even the amount of available material was much too great to permit me as detailed a study as I would have liked to make. And the available material is often unreliable and represents but a small fraction of a huge literature of which new parts may come to light any day, changing our outlook completely.

We may then say that Greek medicine was one of the crafts which supplied some of the chemical knowledge of antiquity. But alchemy proper became imbued with gnostic ideas which stood in contrast to the Hippocratic tradition prevailing in medicine in spite of the inroads made by occult remedies. Until the end of the sixth century we do not find any noticeable connection of physicians with alchemy. At that time, the acceptance of alchemistic doctrines by Neoplatonic philosophers may also have bridged the gulf to medicine, but our knowledge of the events around 600 is too scanty to allow any positive assertions. In the countries of the Islam, however, a different relationship developed. By the middle of the ninth century, chemical techniques including those cultivated by alchemists were so widely spread that they could be used by a physician like Al-Kindī who was adverse to alchemy. From Rhazes we learn that they were also applied to the preparation of drugs. By the end of the century the elixir, the main product of the alchemist, had become endowed with medicinal properties, and thus an approach from alchemy to medicine was affected. In the persons of Rhazes and the legendary Jābir, a union

57-64 and (2), p. 56 ff., has published the French translation of a letter written by a physician Thessalus referring to his student days in Alexandria. Thessalus describes the appearance of the god Asclepius who reveals to him the use of magic remedies for which he had eagerly been searching. Festugière may be right in connecting magic medicine of a popular character with hermetic revelations, and it is not necessary to exempt individual physicians. On the other hand, Cumont's attempt to identify the writer of the letter with the famous Thessalus of the methodist sect seems to me neither proved nor likely. I fully share Diller's (col. 181 f.) suspicion that we are dealing with a later, apocryphal writing.

[81] See the article by Louis Massignon, Inventaire de la littérature hermétique arabe, in Festugière (2), p. 384 ff.

between physician and alchemist is attested and in historical retrospect this union was extended to Greek antiquity. His medical training may account for the lucidity of Rhazes' chemical procedures,[82] and medical theories certainly formed the basis on which Jābir elaborated his numerical speculations. But as alchemists Jābir and Rhazes document a shift in the Hippocratic tradition. The works of the classical philosophers and physicians were transmitted to the Arabic physicians within the framework of late Neoplatonism favorably inclined to Hermetic philosophy. More important perhaps, the countries of the Islam contributed not only new drugs but also a new cultural milieu where ancient doctrines received a new meaning. Jābir represents ancient science in the service of a religious movement while Rhazes was one of the great philosophical and religious heretics in the Mohammedan world.[83] In its practical effects upon medicine, the connection with alchemy was as yet tenuous, but once established it made further developments possible.

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[82] See above, footnote 41.

[83] Rhazes not only represents the Platonic and atomistic traditions as against Aristotle but also appears as a believer in the existence of demons and an opponent of the orthodox Islamic theory of revelation; see Pines, pp. 35, 76, 87 f.; also Heym, p. 187 f. The fact that his clinical observations make Rhazes the most "Hippocratic" of all Arabic physicians, must not mislead us to the belief that his religious and metaphysical views were those of the author of On the Sacred Disease. Discrepancies in this respect are no more surprising than the discrepancy between his chemical exactness and alchemistic conviction.

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J. B. VAN HELMONT'S REFORMATION OF THE GALENIC DOCTRINE OF DIGESTION *

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The replacement of an age-old "thermal" theory of digestion by a chemical theory is not the least noteworthy scientific achievement of the seventeenth century physician and chemist J. B. van Helmont, and is sometimes regarded as one of his most important contributions to science. For a century after his death physiology and pathology were widely regarded as explainable through chemical theories of acid and alkali reactivity which were rooted in Helmont's doctrine of digestion; and when, in the nineteenth century, the acidity of the gastric juice was demonstrated, physicians were again reminded of his pronouncements on this subject. Nevertheless, Helmont's writings are in general replete with an ideology so antipathetic to that of modern science as to make his role in the development of modern science a very problematic one.[1] When his doctrine of digestion is envisaged as part of his whole medical and philosophical doctrine, as Strunz and Pagel have shown,[2] his work cannot be regarded as simply a first step in the evolution of the modern view. The question then arises as to how it happened that this mystically inclined natural philosopher propounded a doctrine at once so plausible as to dominate the thinking of subsequent generations of medical theorists, and so similar to that of modern medicine. This I believe to be most profitably sought in a comparison of his doctrine of digestion with other contemporary doctrines.

As is well known, it was the opinion of Galen and subsequent doctors of "Galenic medicine" that digestion is engendered by heat, through which the nutritious portion of food and drink is successively converted to chyle, to the four humours, blood, phlegm, yellow bile, and black bile or

* Read at the twenty-seventh annual meeting of the American Association of the History of Medicine, New Haven, Connecticut, May 7, 1954.

[1] Garrison takes a relatively favorable view of Helmont's work, calling him "the first to recognize the importance of ferments and gases," (*An Introduction to the History of Medicine*, Philadelphia, 1913, p. 251), but Castiglioni declares that, "his work on close examination does not deserve the high merit sometimes attributed to it. His theories neither explain nor dimly grasp the secrets of vital phenomena." (*A History of Medicine*, New York, 1941, p. 540).

[2] Franz Strunz, *Johann Baptiste von Helmont*, Leipzig, 1907. Walter Pagel, *The Religious and Philosophical Aspects of van Helmont's Science and Medicine*, Baltimore, 1944. (Supplements to the Bulletin of the History of Medicine, No. 2).

melancholy, and is finally assimilated to the body.[3] Digestion, as here defined, corresponds more nearly to what is now called metabolism, or perhaps it would be more correct to say that metabolism was seen by these physicians as a series of digestions. The sixteenth and seventeenth century anti-Galenists used the term digestion in the same way, and we shall here follow that terminology, keeping in mind this difference in its signification.

The Galenic picture was rejected in toto by Paracelsus and his followers. In its place they propounded a new system which is expressed by Oswald Croll, one of Paracelsus' most important disciples, as follows:

> The archaeus of man, or internal chemist, born within and implanted by God, when it takes to itself the food, at once separates the impure feculent tartar from the pure nutriment. Tartar is thus the excrement of food and drink . . . Out of the food, through the digestion of the archaeus of the stomach, that is, separation and generation of the separation, the primum ens comes to life from which comes the nutriment and food of the body. It is reduced into mercury [which occupies the place of nourishment], sulphur, and salt, as is evident in the three principal waste substances.[4]

It does not appear that the Paracelsian theory of digestion was favorably received, except for certain details, outside of what might be called the "hard core" of the Paracelsians themselves. Daniel Sennert, in his judicious review of the controversy between the Galenists and the chemists, takes a very dim view of the Paracelsian doctrine of digestion.[5] Even that eminent chemist J. B. van Helmont, speaking of this doctrine, which he attributes to Paracelsus himself, says, "Neither is it worth one's labor by scoffing at this man to be drawn any longer on the stage, while himself doth infringe his own doctrine:"[6] Sennert tends to adhere to Galen, but he is troubled, as had been Galen himself as we shall presently notice, by the peculiar character which he is obliged to recognize in the heat of digestion, and concludes by classifying it as an "occult quality" along with magnetism and the genius of Alexander.[7] Helmont, on the other

[3] Max Neuburger, *History of Medicine*, vol. I, London, 1910, p. 255.

[4] *Basilica chymica* . . . aucta a Johan. Hartmanno, Venetiis, 1643, Praefatio admonitoria, pp. 134-5.

[5] "Philosophy is calling things by their names, stomach, stomach, liver, cooking power, also by its own name. It is not to mock everything and to confound different things." De consensu et dissensu chymicorum cum Galenicis, *Opera*, Lugduni: J. A. Hugueton, 1676, vol. I, p. 240.

[6] *Oriatrike*, London: Lodowick Loyd, 1662, "A sixfold digestion of human nourishment," p. 219.

[7] Sennert, *op. cit.*, p. 201.

hand, rejects Galen as well as Paracelsus, and his criticism and its origins will be the chief subject of this paper.

Helmont objects first of all to the doctrine of "the Schools," as he calls the contemporary doctors of Galenic medicine, in that "it has brought no light into the art of healing," [8] a remark which reminds us that both he and Paracelsus were greatly disturbed by the tendency of doctors in their time to classify diseases too readily as incurable. He then follows this general objection with a critical analysis of the teaching of the Schools on digestion, in the course of which he points out their errors and expounds his own doctrine. The Schools, he says, "affirm that meat and drink are by the force of heat transchanged in the stomach into a liquor, the which, by reason of its likeness to barley cream, they have called chyle." In the first coction, which they have called the first of three digestions, "that which is more hard and thick [in the food] is excluded: but in the other the thick [part] is retained, the transparent part [urine] being secluded." In "the third place . . . the blood . . . is by degrees perfected and transchanged into a humour." [9] Helmont agrees "without controversy" that it belongs to meats and drinks "to be dissolved into a cream, plainly transparent in the hollow of the stomach." But it is not "by the force of heat," but rather "by virtue of the first ferment, manifestly sour or sharp, borrowed from the spleen: for I have found as many suitable ferments as there are in us digestions." [10] Yet another error is involved in the teaching of the Schools "that the venal blood is made in the liver, and that together with the venal blood and gall [the yellow bile] is also made . . . In that thing I greatly differ from the Schools, because first of all they teach that the gall is not a vital bowel." [11] He holds that the gall aids in preparing chyle for the liver, "for as the ferment of the liver is of a greater work and perfection, so it doth more slowly perform its charge than the ferment of the gall. For the aforesaid transmutation of the cream ought to proceed that the liver being somewhat eased of an unprofitable burden might more commodiously employ itself in sanguinification. Therefore the second digestion, or that of the gall, is distinct from the first and third. . . . All of which the Schools are hitherto ignorant of because erring in the use of the gall." [12]

As will be shortly noted, however, this was not the sole or even the most

[8] Helmont, *op. cit.*, "The threefold digestion of the schools," p. 204.
[9] *Ibid.*, pp. 203-4.
[10] *Ibid.*, p. 206.
[11] *Ibid.*, p. 208.
[12] *Ibid.*, p. 210.

important function of the gall. He goes on to describe four more digestions, the last of which takes place in the "kitchens" of the various parts of the body. Ferments of an obscure nature are involved, but these digestions are nothing like so fully described as the first two. The differences between Helmont and the Schools seem to be mainly in respect to the first two or three digestions—neither of them clearly distinguish the second from the third—and it is doubtful that an analysis of their differences can be—or need be—carried as far as the ultimate assimilation of nourishment.

It is evident from the foregoing that Helmont's objections to the doctrine of the Schools on digestion were part and parcel of a new theory which he proposed to found essentially on two points: (1) that an acid ferment rather than heat was the agent of the digestion of the stomach, and (2) that an important role, in the subsequent digestion, is played by the gall, "a bowl in the form of a liquor [the yellow bile of the Galenists]." In order to attempt to ascertain the significance of this comprehensive reformation of the doctrine of digestion we must first consider the writings of "the Schools," for they were not quite so simple as Helmont makes them. As representatives of the Schools we have chosen Galen himself and one of his leading followers in the sixteenth century, Jean Fernel (Fernelius).

Galen's development of his theory of digestion, in *On the Natural Faculties*, is part of a polemic against the Epicurian atomists who denied that food is essentially altered in character in the digestive and nutritive process. The subduing of the food by the animal implies alteration, he declares.[13] The alteration of food begins in the mouth, which "obviously changes [the food] to a new form," as you may observe by applying masticated grain to the ripening of a boil, which mere water could never accomplish. The alteration in the stomach "is more than that which occurs in the mouth, but less than in the liver and veins." Indeed, there "is no comparison" between the process of mastication and what occurs in the stomach. "We have only to consider what the stomach contains, phlegm, bile, pneuma, heat, and indeed the whole substance of the stomach," to which he adds the influence of "the adjacent viscera, like a lot of burning hearths around a great cauldron." [14]

Against this view "Aesclepiades held that there was no such thing as real qualitative change; the food was merely broken up into its con-

[13] *On the Natural Faculties*, with an English translation by Arthur John Brock, London, 1916, p. 251 (The Loeb Classical Library). This is essentially what Aristotle had held in *Meteorology*, IV, 2.

[14] *On the Natural Faculties*, pp. 251-55.

stituent molecules, and absorbed unaltered." [15] And he denied that partly digested food obtained by vomiting or dissection shows a qualitative change. Erasistratus, says Galen, " is still more foolish and absurd," in his misunderstanding, real or sophistical, of the sense in which the ancients called digestion similar to boiling. That is to say, Erasistratus found it inconceivable that the trifling warmth of digestion should be equated to the boiling process, " as if," says Galen, " we were to suppose that it was necessary to put the fires of Etna under the stomach before it could manage to alter the food." He calls this " quarreling uselessly with a mere name—as though Aristotle had not clearly stated in the fourth book of his *Meteorology*, as well as in many other passages, in what way digestion can be said to be allied to boiling." [15a]

In the passage of *Meteorology* here referred to Aristotle undertakes a general discussion of πέψις. πέψις, or coction, is subdivided into three classes, πέπανσις, by the sun as in the maturation of plants, ἕψησις, elixatio or coction by boiling, and ὄπτησις, assatio or coction by grilling or broiling.[16] In some cases of coction " the end of the process is the nature of the thing," while in others it is some intermediate form, a distinction which the commentator Olympiodoros explains as differentiating digestion proper, where there is a *transformation of the form* of nourishment from such a coction as that of the grape into wine, where there is a simple *achievement of the form proper to the thing*.[17] Again Aristotle declares that " coction by grilling and by ebullition are then operations of art." But " art imitates nature . . . thus the digestion of nourishment in the body is like the coction by ebullition . . ." [18]

Fernelius is in generally close agreement with Galen on all this, and he is troubled by the same problems, such as the difficulty of explaining the inability of the lion, despite its well-known hot temperament, to digest iron, which the ostrich easily manages, and while acknowledging the similarity of coction to cooking, as implied by its name, he emphasizes the importance of distinguishing the " innate heat " of coction from " elemental heat." [19]

[15] Brock, *ibid.*, p. 256, note 2.

[15a] *Ibid.*, pp. 259-61.

[16] *Les Météorologiques*, edited and translated by J. Tricot, Paris, 1941, Bk. IV, 2, p. 232. See especially note 3, p. 232.

[17] *Ibid.*, p. 234 and note 8, p. 234.

[18] *Ibid.*, Bk. IV, 3, pp. 245-6.

[19] *Io. Fernelii Ambiani universa medicina*, Lutetiae Parisiorum: apud Andream Wechelum. 1567, 116-17.

Galen also criticizes Erasistratus for denying a function to the spleen.[20] Neither he nor Fernelius was ignorant of the alleged secretion from the spleen to the stomach of the fluid which they called black bile and which Helmont saw as the ferment of the first digestion. Nor was its reputedly acid character overlooked by them—acidity being vaguely understood but in general taken in the modern sense. Its function in the stomach, as they saw it, was to exercise some control over the portals of that organ, holding the food within it during the period of digestion.[21] Such a contractile action was natural to an acid liquor. But this was only a partial solution to the problem, for to the Schools, as to Helmont, acidity in the body was noxious. This acid liquor, according to Fernelius, represented a superfluous degenerate part of the melancholy humour, which in its natural condition was mild in character. Natural melancholy had some vague "cleansing" function in blood formation and a superfluous part was passed on to the spleen for its nourishment. The acid liquor sent to the stomach was a still more degenerate portion of this. In his ratiocinations on this Fernelius develops a full set of noxious humours, including acid humours from the phlegm and yellow bile.[22] His emphasis on the *unnatural* character of these humours does not strengthen his original argument on the *use* of the acid liquor in the stomach, a point which Helmont dwells upon at some length.[23]

In his discussion of the subsequent transformation of aliment Galen is not entirely consistent on the role of the liver and the veins in the conversion of the chyle into the humours. But both in *On the Natural Faculties* and *On the Use of the Parts* he compares "what happens in the generation of the humours" to the alteration of the fermented grape "through the agency of its contained heat," the flower and lees being compared to yellow and black bile. He is clear in distinguishing this as a mere comparison, but he is equally clear in attributing both phenomena, the digestion in the liver and veins and the fermentation of the grape, to the agency of innate heat.[24] Here Fernelius fails to follow Galen, for he

[20] *On the Use of the Parts*, Bk. IV, ch. 16, *Oeuvres de Galien*, translated by Ch. Daremberg, Paris, 1854, vol. I, pp. 318-19. *On the Natural Faculties*, pp. 143, 205.

[21] Galen, *On the Use of the Parts*, Bk. V, ch. 4, Daremberg, p. 349, Fernelius, *op. cit.*, pp. 122, 133-4.

[22] Fernelius, *op. cit.*, pp. 122, 133-4. Galen wrote in the same vein, but less explicitly, in *On the Natural Faculties*, pp. 209-11.

[23] "For how silly a thing is it, to have feigned the worst drosse of nature (as they confess it to be) and a divorced and abhorred excrement, to be to the stomach for a delight?" "A passive deceiving and ignorance of the schools the humorists," *op. cit.*, p. 1029.

[24] *On the Natural Faculties*, p. 209. *On the Use of the Parts*, Daremberg, vol. I, pp. 281-2.

denies heat to be the agent in the formation of the humours. He considers the agent to be " some intrinsic power of the liver," but he also compares it, almost exactly in Galen's words, to vinous fermentation.[25] The several manifestly different substances, flower, lees, etc., which separate out in the course of vinous fermentation are in both cases at the root of this comparison, but where Galen considered fermentation a kind of digestion, Fernelius, since he denies the agency of heat, appears to have considered the second digestion a kind of fermentation.

Just as the spleen plays in the system of Galen and Fernelius a somewhat secondary role in the first digestion, so does the gall bladder in the second. But its role is even less clearly worked out than had been that of the spleen, in fact it is so elusive as to be practically intangible. Galen seems to give it a function which he also gave to the spleen, the " purification of the blood." [26] Helmont chides the Schools for " erring in the use of the gall," but Sennert accuses them of having failed even to include it among the parts of the body.[27]

Thus the supposed role of heat as a causative agent in digestion is seen to have been recognized as a thorny problem as early as Aristotle, and the efforts expended by " the Schools " in differentiating their " innate heat " from ordinary heat seem deserving of somewhat more acknowledgement than Helmont gives them. The analogy of the wine vat, by which Galen exemplifies the formation of the humours, is strengthened by Fernelius to the point that he appears—although he does not specifically say so—to regard this digestion as a kind of fermentation, and here again the Schools appear to have been closer to Helmont's thinking than he admits. Before venturing to suggest, however, that Helmont's doctrine of ferments was inspired by the speculations of his adversaries, we must give further consideration to his doctrine, for his first two ferments had a much more specified character than had the vague innate heats and powers with which the Schools preoccupied themselves.

The specificity of these ferments is revealed in connection with his assignment of new roles to the spleen and gall bladder, which was perhaps the most original part of Helmont's doctrine. He makes short shrift of the School's tortured elucidation of the acid humours. " The ancients," he says, " smelled out some history of ancient truth: to wit, that whatsoever things, meats being digested, are cast out by vomit, are of a sour taste and smell: yea although they were seasoned with much sugar." [28]

[25] Fernelius, *op. cit.*, pp. 121, 127.
[26] *On the Natural Faculties*, pp. 147-9.
[27] Sennert, *op. cit.*, pp. 238-9.
[28] " A sixfold digestion of human nourishment," *op. cit.*, p. 207.

"Truly they say; that sometimes a black sharp juice is seen to be cast forth by vomit, the which falling on the earth, would lift up the same in manner of a ferment or leaven: whence they have consequently gathered many absurdities. (1) That that sharp or sour excrement ought from its colour, to be called melancholy or black choler . . . Vomits are not the humours boasted of by the Schools: but they are the excrements which the revenging disease frameth, and expelleth." Nevertheless, "there is indeed a sharp, vital, and spiritual ferment in the spleen, whereby the stomach cocteth: the which to wit, fayling, the appetite also goes to ruine." [29] But "the ferment conceived in the cream of the stomach is pernicious [because of its acidity] in the intestine." Now Helmont claims to have observed "that a sour cream in the duodenum doth straightway attain the savour of salt, and doth so willingly exchange its own sharp salt into a salt salt. No otherwise almost than as the vinegar which is most sharp hath forthwith (through red lead) put off its former sharpness, and doth presently change into an aluminous sweetness; . . . or [when] a sharp distillation is drunk up in an alcali salt." [30] It is the ferment of the gall through which this "sharp volatile salt is changed into a salt volatile one." [31]

Here is a clue to Helmont's reformation of the theory of digestion. He compares the processes of digestion to those of ordinary chemistry, especially to the reactions of acids, which were of the greatest interest to contemporary chemistry. Similar comparisons occur in other passages, as where he cites as proof of the speed with which the stomach acidity may be neutralized the "handicraft operation" of the dissolution of mercury in sulphuric acid.[32] This similarity of supposed body acids to mineral acids had occurred to others before Helmont. Valerius Cordus, a century earlier, and after Cordus, Andreas Libavius, had indicated that sulphuric acid was sometimes called "artificial melancholy." [33] But the remarkable consequence of the reaction between acids and alkalis—that is, neutralization—is scarcely noticed in chemical writing earlier than Helmont, yet shortly after him this appears prominently in medico-chemical works. May it not be that it is his observation of the dissolving power of inorganic

[29] "A passive deceiving and ignorance of the schools the humorists," *op. cit.*, p. 1031.
[30] "A passive deceiving" etc., *op. cit.*, p. 207.
[31] *Ibid.*, p. 210.
[32] *Loc. cit.*
[33] "The third part of the De Extractione of Valerius Cordus," edited and translated by G. K. Tallmadge, *Isis*, vol. 7 (1925), p. 397; Andreas Libavius, *Alchymia*, title page missing (Yale University Library, handwritten identification as Francofurti, 1597, edition), p. 340.

acids and of the acid counteracting power of alkalis which leads him to suppose that he has found the key not only to the agency of digestion, but to the real identity of the humoral fluids? It may be objected that this is a mechanistic theory and Helmont was emphatically a vitalist. He has indeed done his best to make this a vitalistic theory, not only by repeatedly stating that the acid ferment in the stomach was not *merely* an acid, but by adopting Paracelsus' archaeus—the internal chemist—to preside over the process, and even by accusing the Schools themselves of an unwarranted transference to the body of the phenomena of inorganic chemistry.[34] Yet his analogies and comparisons are no more clearly distinguished as such than were those of Galen, and even his express differentiations of ferments from acids are often equivocal.[35] It can hardly be doubted that Helmont *believed* his acid ferments to possess a peculiar vitalistic property, but can it be any more doubted that the Schools believed their "innate heat" to possess an equally peculiar vitalistic property? Much of the ridicule Helmont heaps upon the Schools is accomplished by taking literally statements which they evidently intended to be illustrative or analogous, leaving unregarded the reservations which accompanied these statements.[36] Those reservations were in many cases an effort to forestall anticipated criticism, the same objective, it would appear, which inspired Helmont's own protestations against a mechanistic interpretation of his ferments. In the last analysis it was the *acidity* of his first ferment which enabled it to surmount the obstacles to chylification which had plagued the Schools' "innate heat."[37] Beyond its acidity Helmont's first ferment has scarcely any definable properties, and, excepting its relieving the liver of an "unprofitable burden," the acid-counteracting property of the second ferment is its only tangible property. The remaining ferments, to

[34] "But another (Humorist) hath seen salt to be resolved into water, nor to be any longer seen, and presently by the boyling up of the water, that the salt doth again appear; they have therefore supposed the same thing to happen in their own feigned humours." "A passive deceiving," etc., *op. cit.*, p. 1027.

[35] He seems to regard a knowledge of acids as essential to an understanding of ferments, where he says, speaking of the schools' comparison of the action of vomit on earth to fermentation, "But if the Schools had ever poured forth aqua fortis, they had easily found that sharpnesse dissolves earth, but not ferments it. That the action of ferments is one things having a co-resemblance . . . Therefore Galen never knew these things, he never knew aqua fortis, yea not so much as rose water, he never smelt out formal ferments." (*Ibid.*, *op. cit.*, p. 1029).

[36] For example, as we have noted, he takes literally what appears from his own statement to have been a mere comparison made by the Schools between fermentation and the action of vomit on earth.

[37] What powers have ferments beyond innate heat to answer such questions as why the lion cannot digest iron, which the ostrich easily manages?

which he does not attribute any specific chemical property, appear to have no identifiable characteristics at all.

After Helmont the ideas with which he had toiled were developed into a system of medicine distinguished by the predominant role which it gave to the interaction of acids and alkalis in the functioning and malfunctioning of the body. It is ironic that this throughly mechanistic school should have as its founder the fervent vitalist Helmont. Perhaps the explanation for this lies in the circumstance, noted above, that Helmont's *first* objection to the doctrine of the Schools was " that it has brought no light into the art of healing." This we may presume his own doctrine was intended to do, but the only point on which it touches pathology is his frequent insistence on the injurious character of acidity outside of the stomach, and the importance of its neutralization by the bile. And we should note his remark that " Hippocrates spake more rightly than Galen, where he says, ' Diseases are not hot, or cold, etc., but sour, sharp, bitter, and brackish.' " [38] The prominent doctors of the next generation, such as de la Boë Sylvius and Willis, who were influenced by Helmont's views on the pathological effects of body acids, also subscribe to a theory of digestive ferments, and Sylvius reveals that it is still based on the manifest separations which occur in the wine vat. Only the multiplicity of Helmont's ferments, and the vagueness of most of them, permit us to suppose that he may have found some other aspect of fermentation on which to base his theory of digestive ferments. The probability seems rather that his digestive ferments derive ultimately from " the Schools," and that his own contribution, despite his vitalistic predilections, was the combination of this theory with the doctrine of acids and alkalis in the body.

[38] " The Blas of man," *op. cit.*, p. 186.

ANTOINE LAURENT LAVOISIER'S CONTRIBUTIONS TO MEDICINE AND PUBLIC HEALTH *

DENIS I. DUVEEN AND HERBERT S. KLICKSTEIN

Antoine Laurent Lavoisier's contributions to medicine and public health, or more properly hygiene, may be considered from two different approaches: first from his theoretical accomplishments, appropriately termed the new chemistry, particularly in relation to gases, respiration, combustion, and nomenclature; this aspect is primarily concerned with the ultimate changes brought about by Lavoisier's doctrines, although the initial impact is of interest historically.[1] The second approach is to study Lavoisier's own utilization of his discoveries in medicine and public health; this is necessarily an evaluation of the first application of Lavoisier's findings to these fields, and it is noteworthy for demonstrating not only the utility of his chemistry, but also for underscoring his own innate abilities of organization and administration, and his preoccupation with the social problems involved. This presentation will be devoted primarily to the second approach, since it is the least known, and because it gives, perhaps, the clearest picture of the changes wrought in medicine and public health in France by Lavoisier's chemistry and related work in the closing decades of the eighteenth century.

Alone or in collaboration, Lavoisier was connected with some seventy-three reports on twenty-three subjects pertaining to medicine and public health.[2] Of greatest importance were the investigations of prisons and

* Read at the twenty-seventh annual meeting of the American Association of the History of Medicine, New Haven, Connecticut, May 7, 1954.

[1] For details see: A. Pannetier, *Lavoisier et les sciences médicales*, Commentry (Allier), 1895; J. C. Hammeter, *Johns Hopkins Hosp. Bull.*, *29*, 254-264 (1918), and *Janus*, *25*, 1-22, 57-86 (1921); G. A. Lusk in L. F. Barker, *Endocrinology and metabolism*, New York, 1922, vol. 3, pp. 3-78, and *Nutrition*, New York, 1933; A. R. Dujarric de la Rivière, *Strasbourg médic.*, *93*, 199-203 (1933); M. Laignel-Lavastine, *Progrès médic. supp.*, *10*, 25-32, 33-40 (1933); E. A. Underwood, *Proc. Roy. Soc. Med.*, *37*, 247-262 (1944); H. Hartley, *Proc. Roy. Soc.*, *A189*, 427-456 (1947); J. A. Codazzi Aquirre, *Rev. méd. de Rosario*, *33*, 984-990 (1948).

[2] For full bibliographical details of the published works of Lavoisier referred to in this paper see: D. I. Duveen and H. S. Klickstein, *A bibliography of the works of Antoine Laurent Lavoisier, 1743-1794*, London, 1954, and *Catalogue of printed works by and memorabilia of Antoine Laurent Lavoisier, 1743-1794 . . . Exhibited at the Grolier Club*, New York, 1952. All references to the *Oeuvres de Lavoisier* are to manuscripts printed posthumously.

hospitals; these incorporate not only Lavoisier's chemistry but considerations by him of climate, lighting, water purity and supply, sewage, ventilation, and nutrition. The prison report was made in 1780 and the hospital report in 1786; before discussing these inquiries in detail, it is salient to review briefly Lavoisier's scientific endeavors pertinent and prior to these studies so that his methodology may be understood in the proper perspective.

Lavoisier's early work in science, 1763-1771, was devoted mainly to five topics: meteorology, lighting, mineralogy and geology, the water supply of Paris, and hydrometry and the apparent weight and density of pure water.[3] Each of these had a direct bearing on Lavoisier's subsequent appraisals of medical and public health problems. In 1763 Lavoisier made his first meteorological observations and experiments, a pursuit that persisted throughout his life.[4] His knowledge of the effect of climate on man and in agriculture, with the dominant need for an adequate food supply stems from here. Lavoisier's theoretical and practical study in 1765 in connection with the lighting of Paris made him cognizant for the first time of community health and comfort in general and of the effects of illumination in particular.[5] The most profound influence on Lavoisier in this period was the geologist Jean Etienne Guettard, with whom he made a number of field trips for the purpose of compiling a mineralogical atlas of France.[6] Lavoisier not only became familiar with the terrain, a knowledge invaluable to his later researches on water supply, mines,[7] etc., but he saw also the utter poverty of the peasants, their poor health, inadequate housing, lack of food, and hazardous occupations. His later association with the *Ferme Générale*, contrary to the usual belief, made him all the more sympathetic to the acute needs of the underprivileged.

The drinking-water supply of Paris was a problem which was being much debated at the time of Lavoisier's election to the *Académie Royale des Sciences* in 1768. An engineer, Deparcieux, had suggested bringing the pure waters of the river Yvette to Paris, but this project was ridi-

[3] A. N. Meldrum, *Isis, 19*, 330-363 (1933); *20*, 396-423 (1934).

[4] *Extraits des mémoires de Lavoisier concernant la météorologie et l'aéronautique. Publiés par les soins de l'office national météorologique de France*, Paris, 1926.

[5] "Sur les différents moyens qu'on peut employer pour éclairer une grande ville," *Oeuvres de Lavoisier*, Paris, 1865, vol. 3, pp. 1-70; also *ibid.*, pp. 71-102. Lavoisier returned to the subject of illumination in 1781 when he recommended a safe system for the new opera house, the old structure having been destroyed by fire; he proposed a combination of lighting and ventilation, all in one unit (*Mém. Acad. R. Sci., 1781* (1784), 409-420).

[6] Subsequently published in J. E. Guettard and A. G. Monnet, *Atlas et description minéralogiques de la France*, Paris, 1780.

[7] *Oeuvres de Lavoisier*, Paris, 1892, vol. 5, pp. 1-238.

culed by some. Lavoisier, however, realized its soundness, and his defence, which included data on the purification of water by filtration and chemical means, was persuasive.[8] He was to support the contention that diseases were often caused by contaminated water.[9] An extension of the water-supply question was the background of Lavoisier's study of an efficient mechanical pump and system of hydrants to provide fire protection.[10] Lavoisier's interest in water sources led to extensive work in hydrometry, water analysis,[11] and the apparent weight of water and its density.[12] In a continuation of his studies on hydrometry, Lavoisier prepared water as a pure substance and demonstrated conclusively that water could not be transmuted into earth by heating.[13] Lavoisier's clear thinking and talent for concise writing were soon appreciated by his fellow academicians and farmers general. Both bodies appointed him to a large number of commissions and committees whose members nearly always chose him to write their reports.

In 1772 Lavoisier began to study combustion and the calcination of metals.[14] His early experiments on combustion, together with a review of the work of predecessors, were published in the *Opuscules physiques et chymiques* (Paris, 1774);[15] this volume, it is noteworthy, included several chapters on the respiration of animals.[16] From 1772-1787 Lavoisier

[8] "Réponse à un mémoire intituleé: réflexions sur le projet de M. de Parcieux de faire venir à Paris la rivière d'Yvette, par le Père Félicien de St Norbert, carme déchaussé, lue à l'académie royale des sciences, le 26 Juillet 1769," *Mercure de France*, *1*, 139-153 (1769). See also *Oeuvres de Lavoisier*, Paris, 1865, vol. 3, pp. 221-226, 255-260; *ibid.*, Paris, 1868, vol. 4, pp. 68-70.

[9] "Rapport sur l'assainissement des eaux corrompues," *Oeuvres de Lavoisier*, Paris, 1868, vol. 4, pp. 146-147; also *ibid.*, pp. 144-145, 153-154.

[10] "Calculs et observations sur le projet d'établissement d'une pompe à feu pour fournir de l'eau à la ville de Paris," *Mém. Acad. R. Sci., 1771* (1774), 17-44.

[11] *Oeuvres de Lavoisier*, Paris, 1865, vol. 3, pp. 145-205, 206-207, 427-450, 456-460, 563-575; *ibid.*, Paris, 1868, vol. 4, pp. 12-14, 17-20.

[12] *Ibid.*, Paris, 1865, vol. 3, pp. 156-157, 438-439, 451-455.

[13] *Mém. Acad. R. Sci., 1770* (1773), 73-82, 90-107; *Observations sur la physique*, 1771, 78-83.

[14] For details see: E. Grimaux, *Lavoisier*, Paris, 1888; M. Berthelot, *La révolution chimique*, Paris, 1890; M. L. Foster, *Life of Lavoisier*, Northampton, Mass., 1926; J. A. Cochrane, *Lavoisier*, London, 1931; M. Daumas, *Lavoisier*, 1941; S. J. French, *Torch and Crucible, the life and death of Antoine Lavoisier*, Princeton, 1941, and *J. Chem. Education*, *27*, 83-89 (1950); J. R. Partington, *Phil. Mag.*, Commemoration Number, 47-66 (1948); H. Guerlac, *Archeion*, *n.s.*, *30*, 393-404 (1951); D. McKie, *Antoine Lavoisier, scientist, economist, social reformer*, New York, 1952.

[15] A. N. Meldrum, *The eighteenth century revolution in science—the first phase*, Calcutta, 1929.

[16] Première Partie, Chapitre XV, Article IV., De l'air corrompu ou infecté par la respiration des animaux, pp. 124-131; Seconde Partie, Chapitre VII, Expérience Première,

elaborated the new hypothesis that combustion and calcination were dependent on the combination of the metal or combustible with *dephlogisticated* air, or oxygen; the theory was definitely stated in 1777 and was to lead to the overthrow of the *phlogiston* doctrine. Lavoisier's explanation with Laplace in 1783 of the heat evolved in combustion and respiration laid the foundation of thermochemistry, and the former's experimentation on the combustion analysis of organic substances was essential to quantitative organic analysis. The demonstration by Lavoisier that oxygen is absorbed and carbon dioxide evolved in respiration, with slow combustion as the source of animal heat, was fundamental to all subsequent metabolic research.

It is of importance to establish the chronology of Lavoisier's chief memoirs on respiration in relation to the prison (1780) and hospital (1786) inquiries where he applied his results to ventilation problems. Even prior to his experimentation on respiration, Lavoisier was cognizant of the need for proper ventilation, particularly in mines, in consequence, no doubt, of his surveys with Guettard;[17] ventilation was not a new problem, but one which hitherto had been treated by empirical means only, theoretical knowledge still being too inadequate to suggest an explanation.[18] In a review made in 1773 of a book by Morand on coal mining, Lavoisier goes into detail on the ventilation question which he obviously thought of great importance.[19] On May 3, 1777, Lavoisier read a paper to the *Académie*, entitled "Expériences sur la respiration des animaux, et sur les changemens qui arrivent à l'air en passant par leur poumon"[20] in which was presented his first serious study on respiration, and which was to culminate in 1789 in the classic investigations with Séguin.[21] Lavoisier

Effet du fluide élastique dégagé de la craie sur les animaux, pp. 302-303, Expérience II., Effet du fluide élastique dégagé des chaux métalliques sur les animaux, pp. 303-305, Expérience VI., Effet du fluide élastique des effervescences sur les animaux, lorsqu'il a été dépouillé de sa partie fixable par la chaux, pp. 309-310.

[17] "Description de deux mines de charbon de terre, situées au pied des montagnes de Voyes, l'une en Franche-Comité, l'autre en Alsace, avec quelques expériences sur le charbon qu'on en tire. Par M.rs Guettard et Lavoisier," *Mém. Acad. R. Sci., 1778* (1781), 435-441.

[18] G. Rosen, *The history of miners' diseases, a medical and social interpretation*, New York, 1943; L. Teleky, *History of factory and mine hygiene*, New York, 1948.

[19] "Rapport fait à l'Académie Royale des Sciences, par MM. LeRoy & Lavoisier, sur la seconde partie de l'Art d'exploiter les mines de charbons de terre; présentée par M. Morand fils, de la même Académie," *Observations sur la physique, 2*, 68-77 (1773). Morand's book was published in 3 volumes under the above title at Paris, 1768-1779.

[20] *Mém. Acad. R. Sci., 1777* (1780), 185-194. Lavoisier was incorrect, however, when he held that the process of combustion took place in the lungs.

[21] "Premier mémoire sur la respiration des animaux," *Mém. Acad. R. Sci., 1789*

showed that the air *éminemment respirable* (oxygen) was used up during respiration, with the *partie méphitique* (nitrogen) of common air remaining inactive in the process. He also demonstrated that the air *éminemment respirable* was converted into *acide crayeux aériforme* (carbon dioxide). In this memoir the actual experiments on respiration were not extensive; for the most part, they verified what was already known in terms of Lavoisier's theory. A continuation of the experiments was given in a second report.[22] On June 18, 1783, Lavoisier and Laplace presented their "Mémoire sur la chaleur"[23] in which they set forth that respiration was in every way a slow combustion process. Finally, in February 1785, just prior to the hospital investigation, Lavoisier wrote his "Mémoire sur les altérations qui arrivent à l'air dans plusieurs circonstances où se trouvent les hommes réunis en société"[24] which was read to the *Société Royale de Médecine*. Here Lavoisier presented a summation of his efforts, reiterating that atmospheric air was a mixture of two gases, nitrogen and oxygen, and that it was the latter that was vital to life. Lavoisier observed that animals suffered progressive discomfort as the concentration of oxygen decreased and the amount of carbon dioxide increased in the atmosphere in which they breathed.[25] This led him to consider the state of air in crowded rooms. He analyzed the air in several hospital wards and in a theater (*Comédie française*). It was found, in both cases, that the oxygen content of the air was reduced and the pro-

(1793), 566-584; "Premier mémoire sur la transpiration des animaux," *ibid., 1790* (1797), 601-612; "Second mémoire sur la transpiration," *Ann. de chimie, 90,* 5-28 (1814); "Second mémoire sur la respiration," *ibid., 91,* 318-334 (1814). The main factors regulating metabolism, as held by Lavoisier, were delineated in these memoirs: environmental temperature, work output, and food digestion. He showed that the increase in pulse rate was proportional to work, and that oxygen absorption was proportional to the product of pulse rate and the number of inhalations. Body temperature constancy was regulated by respiration, transpiration and digestion. These principles were related to health and disease: fever was nature's method of restoring equilibrium; nature, if left alone, would deal with the disease; contaminated air was the cause of endemic diseases and of prison and hospital fevers; the best treatment was fresh air, free breathing, and a salubrious environment. The purpose and design of clothing was also dealt with by Lavoisier.

[22] "Mémoire sur la combustion des chandelles dans l'air atmosphérique, et dans l'air éminemment respirable," *Mém. Acad. R. Sci., 1777* (1780), 195-204.

[23] *Ibid., 1780* (1784), 355-408.

[24] *Mém. Soc. R. Médec., 1782/1783* (1787), 569-582.

[25] Lavoisier erroneously concluded that the ill effects were due to the accumulation of carbon dioxide rather than the lowering of the oxygen content of the air. The facts are that in the worst-ventilated room the oxygen content may fall from 21 to 20 per cent, and the carbon dioxide rise to .5 per cent. Only when the carbon dioxide reaches about 5 per cent is there distress.

portion of carbon dioxide increased; this was reason enough, Lavoisier felt, for more adequate ventilation. He remarked that he had noted an *impatience machinale* among the members of the audience at the *Académie des Sciences* towards the end of a long session, and he attributed this to the deterioration of the atmosphere; rather whimsically he observed that this was a sad handicap for the last speaker at such a meeting. In his closing remarks Lavoisier pointed out the potential danger of infectious exhalations from the sick in spreading diseases in crowded places, and he stressed the urgent necessity of cooperative medical research to determine exactly how infections were disseminated.

From the foregoing it is evident that Lavoisier had acquired considerable experience in problems of climate, lighting, and the purity and supply of water prior to the prison probe of 1780; he had also established by this time the basis of the new chemistry with its elucidation of respiration as applied to ventilation. The government of France under the old regime prior to the publications of the works by Beccaria (1764)[26] and Howard (1777)[27] had given little consideration to the lot of prisoners. During the first administration of Necker, director-general of finance, in 1776-1781, the government expressed some interest in prisons prompted by the above-mentioned authorities, particularly Howard.[28] On January 26, 1780, on Necker's request, the *Académie des Sciences* appointed a commission to report on a proposed plan for new prisons submitted by Colombier of the *Société Royale de Médecine*, and Moreau, architect of the King. The commission consisted of Lavoisier, Tenon, Leroy, Duhamel, and de Montigny. The report, entirely in Lavoisier's hand, was submitted to the *Académie* on April 5 and September 6, 1780; the full report was in four parts, a

[26] G. B. Beccaria, *Dei delitti e delle pene*, Monaco, 1764. First French translation by André Morellet (Paris, 1766).

[27] *The state of the prisons in England and Wales, with preliminary observations, and an account of some foreign prisons*, Warrington, 1777. The first French translation appeared at Paris, 1788 and was made from the 1784 (3rd) English edition. See L. Baumgartner, *Bull. Hist. Med.*, *7*, 486-534, 595-626 (1939) for bibliographical details.

[28] J. Necker, *Compte rendu au roi*, Paris, 1781, pp. 101-102, and *De l'administration des finances de la France*, 3 vols., n. p., 1785, vol. 3, pp. 166-190, "Recherches relatives aux hôpitaux du royaume"; pp. 191-212, "Réflexions sur l'ordre intérieur des prisons"; A. de Staël Holstein, *Notice sur M. Necker*, Paris, 1820, pp. clviij-clxij, where Necker's reforms of hospitals and prisons are discussed and especial mention of Madame Necker's efforts is made; S. T. McCloy, *Government assistance in eighteenth-century France*, Durham, N. C., 1946, pp. 408-409. See also: Martin-Doisy, *Dictionnaire d'économie charitable . . . .*, Paris, 1855-1864, 4 vols.; L. Lallemand, *Histoire de la charité*, Paris, 1902-1912, 4 vols.; C. Bloch, *L'assistance et l'état en France à la veille de la Révolution . . . (1764-1790)*, Paris, 1908.

main *Rapport*,[29] the only portion published at the time, and a supplement containing three additional sections.[30] Lavoisier's correspondence relative to the investigation reflects the initial difficulties the committee encountered in pursuing its work because of the non-cooperation of the prison authorities, and the interest of the government and particularly of Madame Necker in finding a solution to the deplorable situation.[31]

The *Rapport* dealt with the three main prisons in Paris under the jurisdiction of the Châtelet (a court of justice in Paris); the large and small Châtelet, both originally fortresses, and the For-l'Evêque. The three prisons together had an area of only 522-½ square *toises* (about 3,340 square feet) and held usually from 600 to 1000 people. It was incomprehensible to the committee how so great a number of prisoners could be kept in so small a space. The courts and yards were inadequate, the walls too high to allow free circulation of air, the cells were limited in space with low ceilings and so arranged that air and light only entered with difficulty. The inmates, whose clothing decayed on their backs, were heaped rather than laid on the cots which were of straw that was often rotten. Privies, urinals, and sewers all had ready access to the cells and spread their infectious vapors, as did water which seeped in, covering the floors with stagnant pools. Dirt, vermin, and corruption were everywhere.

It was recommended that the old prisons be demolished and replaced by a new structure in a new location. In this new prison the civil offenders were to be separated from the criminals, and women, debtors, and the infirmaries were further to be segregated. In presenting their plans for the new prison, the commission was guided by four principles which, it was felt, were indispensable for health: (1) cleanliness; (2) an abundant supply of water for washing and drinking purposes; (3) free circulation of air; (4) an appropriate regimen. Lavoisier's hallmark is evident in these proposals and their development.

Cleanliness and water supply were considered together. The lack of water in the proposed location was cited, and it was, therefore, suggested that the waters of the Yvette and the Bièvre be brought in by steam-driven

[29] "Rapport fait à l'Académie Royale des Sciences, sur les prisons, le 17 Mars 1780," *Mém. Acad. R. Sci., 1780* (1784), 409-424.

[30] The supplementary parts were later printed in the collected works of Lavoisier from the manuscript: "Observations sur les prisons actuelles de la Conciergerie," *Oeuvres de Lavoisier*, Paris, 1865, vol. 3, pp. 481-485; "Observations sur le plan de M. Couture pour les prisons de la Conciergerie," *ibid.*, pp. 485-487; "Notes pour servir de supplément au rapport des commissaires de l'Académie Royale des Sciences sur un projet d'établissement de nouvelles prisons," *ibid.*, pp. 488-498.

[31] *Ibid.*, pp. 461-464.

pumps, with rain and wells to supplement the needs. An underground canal surrounding the prison on three sides was set forth as an improvement to the usual open diagonal watermain to carry off sewage and excrement, with vents running to the top of the walls to carry off the odor. Drainage, the inclination of the ground, and ample river water to take away the waste were stressed. The privies were redesigned and to be removed from the cells. The need for careful and frequent washings of the courts, yards, cells, galleries, kitchens, and dining halls was considered indispensable to cleanliness and health. It was further felt that the floor stones should be non-porous, joined with mortar or concrete, and that they should have a drainage angle so that spittle, urine, and putrid excrements would not accumulate. Excessive humidity in the cells was warned against because of the resultant exacerbations of skin diseases. Scabies, scorbut, and venereal diseases (as high as 60% among the prisoners) were the chief problems.

An abundant supply of air, as stated in the third principle, was next examined. It was pointed out that air was an elastic fluid which expanded and became lighter when warmed, to be replaced by colder, heavier air. Respiration formed two elastic fluids, neither of which could sustain life, one lighter (nitrogen), the other heavier (carbon dioxide), than common air; the lighter portion was in a much greater volume. It was important, therefore, to provide for an escape of the lighter and heavier mephitic airs by openings at the top and bottom of the cells, the latter openings also serving as an entry for fresh air. Body heat, it was maintained, would set up air currents and aid in the removal of the noxious portions. Such a system, which was given in detail, was not only healthful but would assure cooling in the summer.

As to point four, the regimen of the prisoners, the commissioners approved of bathing on admittance, shaving of those destined for the dungeons, the disinfection of all clothing by heat and steam to kill vermin, the provision of separate prison garb at intervals, which was to be of cotton instead of wool, because the latter could not be cleaned easily and would be destroyed by moths. Individual cots should be provided. A uniform diet without alcoholic beverages was to be the rule. The prisons were to be disinfected once a year by de Morveau's vaporized hydrochloric acid (*acide marin dans l'état de vapeur*) and the dungeons with each new occupant.[32] Bars and grates should be painted with black oil-paint to

[32] L. B. Guyton-Morveau, *Traité des moyens de désinfecter l'air, de prévenir la contagion et d'en arrêter les progrès*, Paris, 1801. Thomas Beddoes specifically refers to Lavoisier and the use of acid vapors for the disinfection of prisons in his *Hygëia: or*

prevent corrosion. The straw of the cots was to be renewed regularly, since human beings, it was contended, should be treated at least with the same consideration as animals whose chaff is frequently renewed. The *Rapport* closed with a consideration of the relation of a prison to community health. Neglected prisons were, the committee felt, a source of putrid, malignant and pestilential diseases and a menace to the citizens who lived nearby.

The supplement to the prison *Rapport* consisted of two short parts [33] which were devoted to observations on conditions in the Conciergerie—ironic, in that Lavoisier was later to be imprisoned here—and Couture's construction plans for this prison; a third part,[34] almost as long as the *Rapport* itself, was made up of notes in alphabetical order intended as an appendix to the main report. These latter notes were arranged entirely by Lavoisier and covered the following topics: *cachots, emplacement des cordeliers, fenêtres et ouvertures, infirmerie des prisons,*[35] *latrines et fosses d'aisance, maladies contagieuses, promenades couvertes, nettoiement des prisons, nourriture des prisonniers, prisons étrangères, prison militaire, travaux dans les prisons.* In several instances the supplementary comments embodied in the notes were more extensive than the corresponding discussions in the *Rapport*, and in other cases additional recommendations were made. The emphasis, however, was on cleanliness, water and air. Lavoisier's background, both chemical and non-chemical, is most apparent in all the prison proceedings, and indeed, from what is known of Lavoisier's activities when associated with a commission, he was probably responsible for its entire inception and completion; the fact that all the documents were in his handwriting is worthy of consideration.

As a consequence of the prison *Rapport*, the king issued on August 30, 1780, a declaration which, as Howard in his third edition of *The state of prisons* (Warrington, 1784) held, contained " some of the most humane and enlightened sentiments respecting the conduct of prisons " (p. 165). Howard found in 1783 that both the Petit Châtelet and For-l'Evêque were torn down, and he was impressed with the reformation of the prisons,

essays moral and medical, on the causes affecting the personal state of our middling affluent classes, Bristol, 1803, vol. 3, pp. 56-57. For details of method see C.-E. A. Winslow, *The conquest of epidemic disease, a chapter in the history of ideas*, Princeton, 1944, pp. 239-242.

[33] See above, footnote 30.

[34] See above, footnote 30.

[35] Tenon published a separate report on the infirmaries: " Mémoire sur les infirmeries des trois principales prisons de la juridiction du Châtelet de Paris; savoir du For-l'Evêque, du petit et du grand Châtelet," *Mém. Acad. R. Sci., 1780* (1784), 425-447.

particularly their cleanliness, which was far better than in any English prison. A new prison for debtors, the Hôtel de la Force, had also been opened (January, 1782); it was airy, spacious, the courts were clean and well supplied with water, and there was a separation of the men, women, and different kinds of prisoners.

In 1782, the king instructed the *Académie* and the *Société Royale de Médecine* to examine the value of a process suggested by a M. Janin which was purported to destroy the mephitic exhalations of cesspools which had proven fatal to a number of workmen employed in emptying them. The *Académie* appointed a committee, consisting of Larochefoucauld, Macquer, Leroy, Fougeroux, and Lavoisier, who were joined by Abbé Tessier, Hallé, and Fourcroy from the *Société Royale de Médecine*, Larochefoucauld and Macquer being members of both groups. Lavoisier's interest in sewage and cesspools had been already aroused in the prison study, and his experience in that direction was to be again applied to hospitals. The résumé of the committee's findings was published in a small volume which recounts the opening and emptying of two cesspits using M. Janin's process which consisted of nothing more than a treatment of the contents with vinegar.[36] The second experiment resulted in the death by asphyxiation of one workman and the narrow escape of several others so that the procedure was not considered satisfactory. Lavoisier, in a memoir to the *Académie*, dealt in detail with the composition of the obnoxious and dangerous gases emitted by decomposing sewage.[37] He concluded that the main component of the exhalations from fresh excreta was carbon dioxide which made up about $11\frac{1}{12}$ of the total volume of gas; the remaining $\frac{1}{12}$ was marsh gas (methane). He found that, as the decomposition progressed, the proportion of marsh gas rose, until finally almost pure marsh gas was evolved. The end product of the putrefaction was alkaline in nature and effervesced with acids, giving substantially pure carbon dioxide. Lavoisier found that addition of caustic alkali to excreta prevented putrefaction, and that the presence or absence of air did not affect the process. Lavoisier stressed the rigidly quantitative nature of his experiments, pointing out that M. Janin was entirely wrong, and that the treatment of sewage with an alkali, not an acid, was obviously required. He further gave architectural points to be observed in building cesspools, particularly

[36] *Détail de ce qui s'est passé dans les expériences faites par M. Janin, les 18 et 23 Mars, en présence des commissaires réunis de l'Académie Royale des Sciences et de la Société Royale de Médecine*, Paris, 1782.

[37] "Mémoire sur la nature des fluides élastiques aériformes qui se dégagent de quelques matières animales en fermentation," *Mém. Acad. R. Sci., 1782* (1785), 560-575.

a high-stack ventpipe to allow the escape of toxic gases, a procedure, it will be recalled, already set forth in the prison *Rapport*. Lavoisier concluded the memoir with surprise at finding that the decomposition of fecal matter failed to yield any measurable amount of sulfuretted hydrogen, as had been previously thought. Two additional reports by Lavoisier were made on various aspects of the problem.[38]

The shocking state of the hospitals in Paris in general and of the Hôtel-Dieu in particular had been well known for many years previous to the investigation of the latter institution by Lavoisier and his colleagues in 1786.[39] The partial destruction of the Hôtel-Dieu by fire in 1772 was followed by an extended controversy over its reconstruction. Some wanted to abandon the Hôtel-Dieu with the enlargement of other hospitals, but the administrators of the Hôtel-Dieu succeeded in confuting the proposal. The publication of Howard's second edition of *The State of the prisons in England and Wales with preliminary observations, and an account of some foreign provisions and hospitals* (Warrington) in 1780, and a visit to the Hôtel-Dieu in 1783 by the Emperor Joseph II, who was horrified at what he saw, reactivated the movement to remove the Hôtel-Dieu. A plan was submitted in 1785 by Poyet, the official architect and superintendent of buildings, which advocated a new structure.[40] Poyet was attacked by the friends of the Hôtel-Dieu [41] but the government, now aroused, directed the *Académie* to conduct a probe of Poyet's scheme and to report on their findings; Lassone, Daubenton, Tenon, Bailly, Lavoisier, Laplace, Coulomb, and Darcet were the commissioners appointed for this task. Their report, which was published in three parts from 1786 to 1788, supported the accusations against the Hôtel-Dieu but rejected Poyet's recommendation for one large hospital, proposing instead four smaller units.

[38] *Oeuvres de Lavoisier*, Paris, 1868, vol. 4, pp. 323-324, 395-396.

[39] For details on Hôtel-Dieu see: A. Husson, *Etude sur les hôpitaux*, Paris, 1862; M. Möring and C. Quentin, *Collection de documents pour servir à l'histoire des hôpitaux de Paris*, Paris, 1883-1887, 4 vols.; L. Mac-Auliffe, *La révolution et les hôpitaux (Années 1789, 1790, 1791)*, Paris, 1901; S. T. McCloy, *op. cit.*, pp. 181-210. Also see above, footnote 28.

[40] *Mémoire sur la nécessité de transférer et reconstruire l'Hôtel-Dieu de Paris, suivi d'un projet de translation de cet hôpital, proposé par le sieur Poyet, . . .*, Paris, 1785. The first part was by C.-P. Coquéau.

[41] *Relevé des principales erreurs contenues dans le mémoire relatif à la translation de l'Hôtel-Dieu . . .*, [Paris], 1785. This was answered by Coquéau in *Supplément au Mémoire sur la nécessité de transférer l'Hôtel-Dieu de Paris, ou analyse du Relevé des principales erreurs contenues dans cet ouvrage*, Paris, 1786.

In their first report [42] the commissioners considered the actual conditions at the Hôtel-Dieu, citing as well for comparison visits to La Charité, Saint-Sulpice, La Salpêtrière, and Les Invalides, all of which were far superior. They expressed the desire to integrate in their recommendations all of the results of recent discoveries, especially in medicine. As with the prisons, the authorities of the Hôtel-Dieu refused to help. Two criteria were evaluated: was the Hôtel-Dieu sufficient for the needs of Paris, and was it comfortable and salutary? As for the first point, they found the facilities grossly inadequate, since only 2,000 patients could be accommodated at the most, whereas 4,000 places were required, and even the 2,000 had only 1,219 beds to use, 733 large and 486 small. The comfort and healthfulness were discussed in detail. There were too many patients in one bed, sometimes six, with others on the canopy in addition; there might be 615 patients in one room less than 200 square *toises* (about 1,280 square feet), forming a hospital in itself. This condition led to a great deal of abuse: mistakes were made in the administration of food and drugs, corpses were mixed up with the living, contagious were placed indiscriminately with non-contagious cases, etc. For healthy people, it was stated, such a condition would be uncomfortable, for the sick, it was intolerable. A bed was intended for rest, and sleep was deemed essential for the recovery of the patient, but how, the commission asked, was it possible to sleep under such conditions—there were always skin eruptions, vermin, urine, blood, feces, pus, but seldom sleep. The result of mixing fever patients with the others in one bed was considered from the physics of temperature. In fact, there were no stoves in the wards, the heat from the occupants being sufficient to maintain the temperature; but the air as a result was vitiated, stagnant, and polluted and, therefore, aggravating and harmful. In addition, the insane persons were kept in some instances in the same ward with surgical and fever patients, small pox cases were mixed with others, women in various stages of labor were placed as many as four in one bed. Scabies cases were in the same bed with scabies-free patients. The surgical wards and operating rooms were particularly vile, drenched with blood, pus and other fetid matter; they also were on the ground floor and subject to continuous virbration from the traffic in the streets outside. It was no wonder then that surgical deaths were high, almost all trephinees succumbed to meningeal compli-

[42] The first report was initially issued in 4to and 8vo volumes, *Extrait des registres de l'Académie Royale des Sciences. Du 22 Novembre 1786. Rapport des commissaires chargés, par l'Académie de l'examen du projet d'un nouvel Hôtel-Dieu*, Paris, 1786; and reprinted in *Hist. Acad. R. Sci., 1785* (1788), 1-110.

cations. There were yearly recurrent ward epidemics of puerperal fever; the Hôtel-Dieu lost at times as many as half of its obstetric cases as against 1/44 in London, 1/90 in Geneva, 1/110 in Dublin and 1/128 in Manchester. It was, therefore, concluded that the Hôtel-Dieu was a most unsanitary hospital. This was proven by the over-all mortality figures; at the Hôtel-Dieu 1 in 4.5 died, with 1 in 7.5 at the Charité, and 1 in 6.4 at Saint Sulpice. A further comparison was made as to the duration of diseases at the three hospitals: Hôtel-Dieu 42⅘ days, Charité 23 days, Saint Sulpice 23¼ days.

The high mortality was due, according to the commissioners, to two factors: infection and vitiation of the air. In an extended examination of these causes Lavoisier's studies on respiration were summarized, with the 1785 *Société Royale de Médecine* memoir [43] cited as the authority. A consumption of 5 cubic feet of air per hour for man, as given by Lavoisier, was accepted. The amount of air required by a patient varied with the disease. Inflammatory diseases, small pox, scurvy, insanity, and all conditions associated with high fevers needed high ceilings to provide more air. Senility, weakness, humoral and pituitous cases, humid asthma and intermittent hay fevers required less air and, therefore, low ceilings. The volume of air needed in each category was calculated, and the Hôtel-Dieu was shown to be markedly deficient in addition to the absence of effective ventilation. A point near asphyxiation was found in some wards. Multiple floors (over three) were condemned, since noxious air would accumulate in the upper floors, producing headaches and breathing difficulties; similar experiences in theaters were also noted. The explanation, in terms from Lavoisier, was that vitiated air was lighter than ordinary air and would rise; the vitiated air was charged with disease-carrying miasms.

The second report [44] dealt with the recommendations for sites on which the projected four new hospitals should be built. Water, air and sewage were the primary considerations. The third report [45] gave construction

[43] See above, footnote 24.

[44] First issued in a 4to volume *Extrait des registres de l'Académie Royale des Sciences du 20 Juin 1787. Rapport des commissaires chargés, par l'Académie, des projets relatifs à l'établissement des quatre hôpitaux*, Paris, 1787; and reprinted in *Hist. Acad. R. Sci. 1786* (1788), 1-12.

[45] First issued in a 4to volume *Extrait des registres de l'Académie Royale des Sciences du 12 mars 1788. Troisième rapport des commissaires chargés, par l'Académie, des projets relatifs à l'établissement des quatre hôpitaux*, Paris, 1788; and reprinted in *Hist. Acad. R. Sci., 1786* (1788), 13-41. A Spanish translation by V. de Foronda appeared in 1793 (Madrid).

details for the proposed buildings. A review of hospitals in England was first given, based on a trip made by Coulomb and Tenon; the latter subsequently published a minority report.[46] English hospitals were found to be generally good. The final plan for a hospital as presented by the commission was in all essentials "modern" and could be just as effectively applied today.

Although the king ordered the four new hospitals built on June 22 and August 22, 1787,[47] the project was abandoned amid the turmoil of the Revolution, and the Hôtel-Dieu still stands to this day on its old site. It is of interest, however, that the Glasgow Infirmary was constructed with "every part . . . , as much as possible, according to a report to the late king of France, by a committee of the academy," [48] and a Spanish translation of the third report was also published.[49] Of importance in both the prison and hospital reforms are the progressive ideas on community health developed at that pre-revolutionary period, a fact lost sight of in many of the evaluations of French history, which date the majority of developments only from the Revolution.

Lavoisier's own part in the hospital investigation is evident from the emphasis on water, air, sewage, and the over-all stress on ventilation and cleanliness. He also had at his disposal a manuscript diary, *Voyage en Angleterre, 1774*, by Balthazard Paulze, a brother of Madame Lavoisier, in which some observations on English hospitals were recorded.[50] Thomas Beddoes joined Lavoisier, Fourcroy, de Morveau, Chaussier, and others at the Hôpital de Dijon in 1787, at which occasion the question of the efficacy of de Morveau's method of disinfection by the help of hydrochloric acid gas and chlorine was discussed in detail.[51] Lavoisier, in connection with his interest in hospitals and as a consequence of his hydrometry background, performed a large number of experiments to determine the relationship between the food content, or gelatinous matter, of meat broth and its specific gravity.[52] In another memoir he lists the

[46] *Mémoires sur les hôpitaux de Paris*, Paris, 1788; two approbation reports from the *Académie* in the work (pp. 453-462, 467-468) were signed by Lavoisier among others. See N. W. Faxon, *Bull. Am. Hosp. A.*, *4*, 96-108 (1930).

[47] M. Möring and C. Quentin, *op. cit.*, vol. 2, pp. iv-v, 26-28, 35-38, 63, 114, 125, 163-164, 172-173, 190-191, 199-205, 232.

[48] T. Beddoes, *Contributions to physical and medical knowledge, principally from the west of England*, Bristol, 1799, pp. 16-19.

[49] See above, footnote 45.

[50] Ms., 2 vols., library of Denis I. Duveen, New York City.

[51] T. Beddoes and J. Watt, *Considerations on the medicinal powers and the production of factitious airs*, London, 1796, p. 163.

[52] "Mémoire sur le degré de force que doit avoir le bouillon, sur sa pesanteur spécifique

relative rates of decomposition of different meats,[53] and as a member of a committee to probe the slaughter houses of Paris, he again stressed cleanliness.[54] In relation to foodstuffs, Lavoisier had occasion to report on poppyseed oil,[55] rancid oils,[56] the adulteration of cider,[57] the purification of sugar,[58] baking,[59] the spoilage of grain and flour [60] and the use of zinc, tin, silver, and copper on cooking utensils.[61] Soap [62] and wheel chairs [63] also came under his surveillance. Finally, in 1794, he drew up the published program for a prize offered by the *Académie* for a work on nutrition.[64] This program was a forecast of some of the major developments in physiological chemistry for the next century.

The intuitive genius of Lavoisier in the application of chemical facts and scientific methods to questions of medicine and public health can be gauged as well in his study of ether,[65] the exposé of animal magnetism,[66] the effect of rouge on the skin,[67] industrial hygiene in mines,[68] as already

et sur la quantité de matière gélatineuse solide qu'il contient," *Oeuvres de Lavoisier*, Paris, 1865, vol. 3, pp. 563-575.

[53] "Rapport sur un mémoire du Marquis de Prunelay, relatif à la putréfaction," *ibid.*, Paris, 1868, vol. 4, p. 398.

[54] "Rapport des mémoires et projets pour éloigner les tueries de l'intérieur de Paris," *Hist. Acad. R. Sci., 1787* (1789), 19-43.

[55] *Oeuvres de Lavoisier*, Paris, 1868, vol. 4, pp. 98-102.

[56] *Ibid.*, pp. 238-241.

[57] "Rapport concernant les cidres de Normandie," *Mém. Acad. R. Sci., 1786* (1788), 479-506; "Rapport sur la falsification des cidres," *Hist. [et Mém.] Soc. R. Médec., 1786* (1790), 159-166.

[58] *Oeuvres de Lavoisier*, Paris, 1868, vol. 4, pp. 478-483.

[59] *Ibid.*, pp. 470-473.

[60] *Ibid.*, pp. 317-322.

[61] *Ibid.*, pp. 260-262, 389, 503-507.

[62] *Ibid.*, pp. 125, 301-304.

[63] *Ibid.*, pp. 53-54.

[64] "Prix proposé par l'Académie des Sciences, pour l'année 1794," *Hist. Acad. R. Sci., 1789* (1794), 24-30.

[65] "Réflexions sur les effets de l'éther vitriolique et de l'éther nitreux dans l'économie animale," *Hist. [et Mém.] Soc. R. Médec., 1781* (1785), 426-430. For details see J. F. Fulton, D. I. Duveen and H. S. Klickstein, *J. Hist. Med. & Allied Sci., 8*, 318-323 (1953).

[66] *Rapport des commissaires chargés par le Roi, de l'examen du magnétisme animal*, Paris, 1784, in 4to and 8vo editions; reprinted in *Journal de Médecine, 62*, 449-536 (1784 [1789]); *Exposé des expériences qui ont été faites pour l'examen du magnétisme animal*, Paris, 1784, in 4to and 8vo editions; reprinted in *Hist. Acad. R. Sci., 1784* (1787), 6-15 and *Journal de Médecine, 62*, 561-576 (1784 [1789]). For details see D. I. Duveen and H. S. Klickstein, "Benjamin Franklin (1706-1790) and Antoine Laurent Lavoisier (1743-1794), Part II.," [in press].

[67] "Rapport fait à l'Académie des Sciences, sur le fard," *Observations sur la physique, 6*, 322-325 (1775).

[68] See above, footnote 19.

noted, in addition to his observations on the hazards of mercury in hat making [69] and the toxity of air in wells,[70] vivisection,[71] social hygiene, where he advocated and foresaw the benefits of old-age and health insurance,[72] and the public instruction of children in hygiene.[73]

Lavoisier, as has been shown in the above presentation, primarily from his work on prisons and hospitals, applied his extensive scientific knowledge, chemical and otherwise, profitably to medical and public health problems, always with an awareness of the social factors involved—climate, lighting, the purity and supply of water, air and ventilation, sewage, disinfection, nutrition, food supply and adulteration, mesmerism, occupational diseases, social medicine, public health education. It is not surprising, then, that some of the subsequent advances in medicine and public health were a consequence and logical continuation of Lavoisier's initial efforts.

ACKNOWLEDGEMENT

The authors would like to express their appreciation to Martha Alexander for her assistance in the preparation of this paper.

[69] *Oeuvres de Lavoisier*, Paris, 1893, vol. 6, pp. 12-15.
[70] *Ibid.*, Paris, 1868, vol. 4, pp. 395-396.
[71] *Ibid.*, pp. 9-11.
[72] *Procès-verbal des séances de l'Assemblée Provinciale de l'Orléanois, tenue à Orléans aux mois de Novembre et Décembre 1787*, Orléans, 1787, pp. 270-281.
[73] *Réflexions sur l'instruction publique, présentées à la Convention Nationale par le Bureau de Consultation des Arts et Métiers*, Paris, 1793.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

Medical History in Buffalo

The Editor wishes to thank Dr. Samuel X. Radbill, Secretary of the American Association of the History of Medicine, and Dr. Irving Wolfson of Buffalo, N. Y., for making the following letter available for publication in the Bulletin.

Dear Doctor Radbill:

The following is a brief sketch of the course in Medical History in which I am interested.

Last year at the request of students and one of the faculty members I started two groups of students, one of Juniors and one of Seniors, in a series of seminars or discussion periods. The plan is similar to the time-tested method which is used by the Great Books Foundation.

Each member of the group, students and leaders, reads the assigned text and then meets with the other members for a two hour discussion. The discussion is limited to the text and its significance in history and its influence on present day medicine. While the leaders ask the provocative questions and attempt to keep the text in the foreground the students are encouraged to carry the weight of the discussion. Each group meets for a two hour session once each month at the convenience of the leaders and the students, at the home of one of the leaders. Each student joins the group on a voluntary basis and no records of attendance are kept. Most of the students participating have part-time jobs as clinical clerks at local hospitals and a regular schedule is difficult to follow. The leaders make a special effort to maintain an atmosphere of informality in which the student can express his opinions as candidly as he would in his own quarters.

Our greatest limitation is the availability of texts. As far as possible we have limited our reading to original texts (or translations). We have read Hippocrates, Harvey, Pare, Claude Bernard and Freud. We are building our library as other important texts become available.

It has been an enjoyable experience for leader and student, and an active interest in the History of Medicine has been aroused in both. The students are offering to prepare short biographical sketches of the authors and short reviews of the times when the original paper was written.

I feel the student has gained an insight of the men, their great writings and discoveries. All of us, leaders and students, have noticed a growing and glowing pride in the Heritage of Medicine given to us by our predecessors.

Chiefly, it has been a lot of fun.

Sincerely yours,

Irving Wolfson, M. D.

ANNOUNCEMENTS

AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE
TWENTY-EIGHTH ANNUAL MEETING
DETROIT, MICHIGAN, MAY 12, 13 AND 14, 1955

NATIONAL NEWS

Armed Forces Medical Library

As announced in its *News* of March 1955, the Armed Forces Medical Library is trying to build up " a large and integrated collection " of medical motion pictures " to take their place with other important primary materials for research in the medical sciences and in the history of medicine." The Library is appealing for the donation of obsolete prints and hopes that it will be helped " to recover historical material which has been temporarily lost to medical science and the history of medicine."

Baltimore

The Johns Hopkins Medical History Club met on March 1, 1955. Papers were read by John F. Fulton (" Pre-Harveian Students of the Vascular System ") and John K. Rose (" A 19th Century Medical Missionary—Peter Parker ").

Chicago

The Society of Medical History of Chicago is meeting on March 8, 1955. Alfred Koback will speak on " Evolution of Caesarean Section," A. Rostenberg on " The Development of Knowledge Concerning Poison Ivy," and B. Barker Beeson on " Joseph Souberbielle, Surgeon and Revolutionist."

College of Physicians of Philadelphia

The College held a meeting on February 24, 1955, with the following program: Frederick H. Leavitt, " Stephen Gerard, a Medical Paradox "; Fred B. Rogers, " Dr. John Brown, Friend of Benjamin Franklin "; Louis Gerstley, III, " The Fetal Monstrosity in Medical History."

The Section on Medical History will present a series of seven seminars on the methods of teaching medical history at the Hall of the College, 19 S. 22nd St., Philadelphia, Thursday evenings, 7-9 p. m., March 3, 10, 17, 24, and 31, and April 7 and 14. These seminars will be presented by Owsei Temkin and Richard H. Shryock of the Institute of the History of Medicine of the Johns Hopkins University. It is proposed to have informal talks reviewing questions relating to the interpretation and presentation of medical history to the medical students of Philadelphia. This series of seminars is financially supported by the medical schools of Philadelphia and representatives from each of these schools have been invited to attend. Any Fellow of the College who cares to attend will also be welcome.

History of Dentistry

The *Bulletin of the History of Dentistry* (February 1955) states that of 45 American and Canadian schools replying to a questionnaire sent out by a committee, 39 reported the existence of a required course in the history of dentistry. In 28 schools the course is taught independently from other courses. The number of hours devoted to the subject ranges from 5 to 66.

Madison, Wisconsin

Dr. George Urdang, Emeritus Professor of the History of Pharmacy, will be employed by the University of Wisconsin under an arrangement that will allow continuation of his investigation of old literature for reputed cancer drugs. (*See J. A. M. A.*, vol. 157, February 5, 1955, p. 518).

University of Kansas

The Clendening Lectures on the History and Philosophy of Medicine for 1955 will be given by Dr. Esmond R. Long who will speak in Lawrence, April 12, on "The Illness and Medical Care of Frédéric Chopin" and in Kansas City, April 13, on "The Therapy of Tuberculosis from Hippocrates to Modern Times."

NEWS FROM ABROAD

Deaths

Max Neuburger, M. D., Ph. D., died in Vienna on March 15, 1955. Dr. Neuburger was Professor Emeritus of the medical faculty of the University of Vienna and an honorary member of the American Association of the History of Medicine. The *Bulletin* plans to publish an obituary of Dr. Neuburger in a later issue.

Dr. Erik Waller died on January 28, 1955, at his home in Lidköping, Sweden, at the age of 79. World famous as a collector of works, mainly those relating to the history of medicine, Dr. Waller in 1950 had donated his holdings to the Library of the University of Uppsala (see this *Bulletin*, vol. 25, 1951, p. 90). A catalogue of the *Bibliotheca Walleriana* of about 21,000 works is to be published in the near future. Dr. Waller was a frequent visitor at medico-historical congresses, and all those who had the good fortune of meeting him will remember the charm of his personality, his unassuming scholarship, and his love for the old documents of his profession.

BOOK REVIEWS

RALPH H. MAJOR. *A History of Medicine.* Two volumes. Springfield, Ill.: Charles C Thomas, 1954. xxv + xii + 1155 pp. Ill. $4.50.

In his preface to this excellent and welcome work, the author, after a brief reference to his predecessors in the field, confesses that " it takes some courage to project still another history of medicine." He need have had no such fears; there is ample room for his treatise and the success of his venture is certain. The literature of medical history does not exceed the demand, and sound, scholarly work such as this will always find numerous readers. At the present time there is every need for a comprehensive history of medicine, not so long or so overburdened with detail as to be simply a dictionary for reference, and not so short as to be merely an entertaining narrative. With that skill which we should expect of him, Dr. Major has avoided each of these pitfalls. He acknowledges that he is writing for the medical student or practitioner and not for the specialised historian. He is, therefore, careful to preserve the flow of his story, selected from the vast material now available. Confronted by an immense field which is so closely associated with the social, economic, philosophic, and religious aspects of human existence, the task of the historian is one of selection and analysis, a process which becomes different with each succeeding generation. Every age, therefore, has its own approach to history. The conception of history changes, not only because the advance of time has turned over a fresh page, but also because each generation has its own way of looking at things. A history of medicine is not simply a record of the events of the past. It is, or ought to be, a critical survey of past happenings in the light of present-day knowledge. The book under review, which is obviously the fruit of many years of intensive study, amply fulfils all these requirements.

It is a two-volume work of 1155 pages, well written and beautifully illustrated. The chapters are not numbered. Indeed, the word chapter is not used to designate the various sections, which, however, are clearly demarcated. No extensive bibliography has been attempted, although at the end of the second volume there is a general bibliography of 25 pages, comprising several hundred works written, for the most part, during the present century, and grouped according to the historical periods to which they refer.

Commencing with " The Dawn of Medicine," the author proceeds to deal with Greek and Roman Medicine, the Middle Ages, the Renaissance and the Seventeenth Century. The second volume is devoted to the Nineteenth Century, The Rise of American Medicine and the Twentieth Century. The opening chapter on the early days of medicine, always an attractive theme, is particularly so in the present work. It includes an account of the Inca, Maya, and Aztec Civilizations of Central and South America, but there is little reference to primitive medicine as it exists today in Africa and elsewhere, nor to the Folk-medicine which survives in many parts of the world, even in highly civilized communities. Much interesting information is included on the subject of early medicine in Egypt, India, and China. The dawn days of any branch of knowledge are always of importance. As might be expected

from one who has already written a fascinating report of a journey to the Island of Cos, the passages relating to Greek Medicine are most illuminating. The attention of the reader continues to be held by the clear and lucid account of the Arabian contribution to medicine in the earlier period of the Middle Ages, a time of great intellectual activity, although the energy was so often distracted by useless preoccupation with alchemy and astrology. When the Universities came into being during the later medieval period, the names of the pioneers begin to emerge more clearly. They were men of far vision, as, for example, Lanfranchi of Milan who, early in the 14th century, deplored the separation of surgery from medicine, and Theodoric, of slightly later date, who, at Bologna, held that there was no greater error than to believe that "pus should be generated in wounds as a normal accompaniment of healing." The ravages of the Black Death are well recorded, together with the peculiar sequels, the Flagellant Processions and the Dancing Mania. The Renaissance period is introduced by an account of contemporary history, which enables the reader to grasp more clearly the significance of the medical achievements of that time. Among the leading figures mentioned are Benivieni, who blazed the trail of pathology, and Baillou, who must share with Fracastoro the credit of establishing epidemiology as a science. Passing to the 17th century, we find Santorio (Sanctorius) making use of instruments of precision in medicine, and Harvey creating a complete revolution of outlook by his monumental discovery. The Royal Society, coming on the scene about 1645, did much to stimulate scientific investigation.

The second volume begins with the 18th century, and we now find our author perplexed, as he may well be, by the multiplicity of great names deserving of mention. To include them all, would destroy the flow of his narrative, and accordingly, as in the first volume (although in the second, the problem has become more acute), he solves the difficulty by the use of Biographical Addenda at the close of each chapter or section. In the first volume 38 pages, and in the second, 102 pages, are devoted to those collections of brief biographies, which are of much value as references, although, strangely enough, the dates, and not the names are in heavy type. One wishes that the 18th century had been given greater prominence, with fewer details of Brunonians and Mesmerists, and a fuller consideration of Percival Pott and William Cheselden in London, and of the foundation of the Medical Faculty in Edinburgh by Monro primus. The foremost London physicians, Fothergill, Lettsom, and Mead, have all been relegated to the Addenda, and there is no mention to Benjamin Bell, the outstanding surgeon of the century whose descendant, Joseph Bell, was the surgeon who inspired Conan Doyle to create Sherlock Holmes. Nor do we read of the anatomist Robert Knox, whose brilliant career was shattered by Burke and Hare. Nevertheless, one has every sympathy with the author in his dilemma, and one's admiration of his skill increases as one proceeds to his account of medicine in the 19th and 20th centuries. The pages on The Rise of American Medicine reach a very high standard and will appeal to every reader. Dr. Major, however, has shown no national bias, but has cast his net world wide in his effort to deal fairly with the achievements of every country and nation. The predominance of Paris as the centre of medical education during the first half of the 19th century, and of Germany during the second half of that century, are very well portrayed, while due significance is accorded to the changes in surgical practice which resulted from

anaesthesia and antisepsis, and the rise of bacteriology. The final section of the book, relating to the 20th century, must have been very hard to write, because it is no easy task to assess contemporary history at its true value. With great tact and skill, the author has contrived to link his account of the earlier days to a brief conspectus of present-day medicine. He has wisely avoided entering the medico-political field, which must await the pen of some future historian.

No review of this handsome work would be complete without some reference to the illustrations which form such an arresting feature of both volumes. They are numerous, appearing on almost every page, they are most interesting and well-chosen, and they have been clearly produced. Many are from photographs by the author himself, who has often travelled great distances in search of the material.

In a work of this nature, errors are inevitable, but they are astonishingly few. Lister taught at the Royal College of Surgeons in Edinburgh and had no official connection with the Physicians (p. 822). Cox-pox should be cow-pox (p. 607); Findlay in the text, ought to be Finlay, as in the caption (p. 863, 864); Fuch (p. 927), Gales (p. 1092), and Almoth Wright (p. 982) refer to Fuchs, Gale and Almroth Wright; while Liston was Robert, not James. The Index of Names is excellent, but the Subject Index might have been more descriptive, rather than a repetition of proper names under subject headings. Print and paper are very good, but the binding is ill-chosen. Rough buckram, far from elegant, is simply a dust-trap and will be most unpopular with librarians and housewives. Those slight defects may be easily remedied in future editions.

Admirably planned and well executed, Dr. Major's scholarly History of Medicine is sure to take its rightful place as a leading and authoritative contribution to a noble theme.

DOUGLAS GUTHRIE

FREDERICK B. TOLLES. *George Logan of Philadelphia*. New York: Oxford University Press, 1953. xix + 362 pp. Ill. $5.00.

George Logan (1753-1821), a Philadelphia Quaker, claims only the so-called truant's share of medico-historical attention. That is, he obtained an Edinburgh M. D. degree in 1779, practiced medicine in Philadelphia for about three years after his return (1780) to this country, then abandoned it for the life of a gentleman farmer on the Logan estate not far from the city. It is his subsequent turbulent career—as agriculturist and politician—that amply justifies this first modern, full-scale biography of him.

Under the circumstances, it is understandable that a single chapter largely suffices for an account of the medical aspects of Dr. Logan's career. On his arrival in England, in June 1775, that unfailing friend of young American medical students, Dr. John Fothergill, soon established the young man in the home of a Quaker physician in a little town in Essex. After this apprenticeship, lasting about a year, Logan spent a month in London, two months on a tour of the west of England, and arrived in Edinburgh, always his goal, in November, 1776. There for three years—interrupted by holidays and attendance on a two months' course of William Hunter's lectures, in London—he entered actively, and with delight, into the academic and

social life of the city. In 1779, he was chosen to be the first president of the Medical Society under its new royal charter, the first American ever elected to the office, an honor which he prized more highly than the M. D. degree that was to come a few months later, elated though he was over obtaining the latter.

On completion of his studies in Edinburgh, Logan spent the summer and fall on the Grand Tour, the winter in attending anatomical lectures in Paris. While there, he often visited Benjamin Franklin, at Passy. It was in the final months of this 5-year stay in Europe that there occurred the political awakening which ultimately was to play a large part in changing the course of Logan's career.

In this *Edinburgh and Passy* chapter, whose outlines do not vary greatly from those we are accustomed to in accounts of the European experiences of 18th century American medical students, the author has been well served by Logan's manuscript journal, now in the Historical Society of Pennsylvania. His references show, however, that studious attention was given other unpublished as well as published sources.

As for the rest—the major part—of Logan's career, it lay, as has been indicated, in fields other than medicine. Dr. Logan was a fighting conciliator who knew practically every American and many Europeans of historical importance in his time. Devoted to peace and " the vision of America as an agricultural Eden, peopled by a happy, independent, prosperous yeomanry," he labored ceaselessly for these objectives with fanatic, humorless energy. Somewhat ironically, his name chiefly survives today in a legislative Act (1799) of which he was not the author but the object: the so-called ' Logan Act,' which prohibits private individuals from entering into correspondence with a foreign government on matters pending between the two countries.

Analogies to our own times are not marked out by the author (a Quaker historian and permanent research fellow at the Huntington Library), but will be poignantly obvious to the reader with an interest in politics. A few of these readers will possibly regret the author's attempt " to see things, as far as possible, through Logan's eyes . . . to convey, as far as may be, some sense of how it felt to be George Logan." This sort of thing requires, of course, creative sensibility of a high order, and the most exquisite literary taste, if anything but bathos is to be achieved. It would seem wiser for historians, generally speaking, to leave it in the hands of the novelists, who will normally do it better and whose artistic failures do not innocently involve other values.

As the disposition of references is of some concern to historians, there is regret at having to report that here again the reader interested in the documentation will frequently be under the necessity of turning back page after page to find out the number of the chapter he is in, in order to locate the reference in the *Notes* at the back of the book. Presumably this practice represents both a measure of economy and further solicitude for the large public which becomes uncontrollably restless at the sight of a footnote. What a stroke of sheer genius it would be for the publisher using this disposition of references to slip the chapter-number into one of the running heads. The paper, type, and design of the book are excellent, as was the proofreading; the 12-page index demonstrates a like expenditure of care and skill.

W. B. McDaniel 2d

BERNHARD MILT. *Franz Anton Mesmer und seine Beziehungen zur Schweiz. Magie und Heilkunde zu Lavaters Zeit.* Zürich: Druck Leemann AG, 1953. iv + 140 pp. 7 Swiss francs.

The synonymous use of the chronologically later word "mesmerism" for the earlier term "animal magnetism" in discussions of the doctrines of Franz Anton Mesmer is typical of the uncritical historical treatment of this subject, particularly the initial phases. With but few exceptions this has been especially true of the contributions to the English literature.[1] Early French sources as well as somewhat later German studies provide the only accurate accounts. In the latter group the recent volumes of Rudolf Tischner and Karl Bittel,[2] and Emil Schneider[3] are the most useful and authoritative. To these must now be added this book by Bernhard Milt which is an evaluation of Mesmer in his relationship to Switzerland.

Mesmer's active role in the history of animal magnetism ended with the publication of the unfavorable report on his practices by the joint commission of the academies of science and medicine at Paris in 1784. He was destined, however, to survive the condemnation for thirty-one years and in the end see a rebirth of his ideas. In this period, Mesmer traveled to England, Switzerland, Germany, Italy, and elsewhere and he also returned for an extended period to France. Finally, in 1803, he settled in Frauenfeld, Switzerland, near Lake Constance, remaining here until 1814 when he moved to Mursburg where he died in the following year. During his residence in Switzerland, Messmer joined in community medical affairs and continued to work on his theory. However, the real development and promulgation of his doctrines were accomplished by others, and this was most stimulated by Puységur's characterization of the phenomenon of somnambulism. C. Lavater in turn carried Puységur's concepts into Germany where they soon found a ready audience. Mesmer, in Switzerland, followed the new outgrowth with interest if not satisfaction. He was visited by a number of disciples in Frauenfeld; L. Oken, K. C. Wolfart, and C. A. F. Kluge were the most important and influential. The latter two coined the word mesmerism. Mesmer was in fact rediscovered during his last years in Switzerland.

The activities of Mesmer and his associates with the Swiss hitherto have not been studied in detail; and they certainly are of importance in view of the rejuvenation of his practices during his stay in Switzerland. B. Milt has traced through manuscript sources not only Mesmer's life in Switzerland but the reception of animal magnetism by the Swiss both before and after his emigration from France; this is further related to the attitude in the other countries visited by Mesmer. The controversies over animal magnetism and subseqently somnambulism in Switzerland are followed and the resurgence of his influence evaluated in terms of German as well as Swiss reactions.

HERBERT S. KLICKSTEIN

[1] An excellent short survey is given by G. Rosen in J. M. Schneck, *Hypnosis in modern medicine*, Springfield, Ill., 1953, pp. 1-27.

[2] R. Tischner and K. Bittel, *Mesmer und sein Problem. Magnetismus-Suggestion-Hypnose*, Stuttgart, 1941.

[3] E. Schneider, *Der animale Magnetismus. Seine Geschichte und seine Beziehungen zur Heilkunst*, Zürich, 1950.

HERBERT CONWAY and RICHARD B. STARK. *Plastic Surgery at The New York Hospital One Hundred Years Ago. With Biographical Notes on Gurdon Buck.* New York: Paul B. Hoeber, Inc., 1953. x + 110 pp. Ill. $5.00.

The chief value of this little book is found in the biographical sketch of Dr. Gurdon Buck. He is best known for his revival of the use of traction in the treatment of fractures of the femur. His method, known as Buck's extension is still in use. His improved method of treating fractures of the thigh was reprinted in *Medical Classics* in 1939. Much more important were Buck's contributions to the literature of plastic and reconstructive surgery which, while not entirely original, were far in advance of his time. His book entitled *Contributions to Reparative Surgery*, was published in New York in 1875.

Dr. Buck began his work in plastic surgery during the Civil War as a member of the Sanitary Commission. There are some interesting sidelights on the treatment of the wounded during the Civil War.

The statement that the New York Hospital was active in the care of the wounded in the Revolutionary War is open to question since the British used it as a barracks for troops during the long occupation. The surgeon, John Jones, the only member of the staff who sided with the Americans, left for Philadelphia at the outbreak of war and never returned to New York.

The fifty references in the bibliography are mostly on Buck's contributions to the periodical literature. The local history references are to secondary sources. The book contains some unusual pictures of the New York Hospital. There is no index. The book is a welcome contribution to the history of plastic surgery and to the history of medicine in New York City.

CLAUDE S. HEATON

PAT IRELAND NIXON. *A History of the Texas Medical Association, 1853-1953.* Austin: University of Texas Press, 1953. xx + 476 pp. Ill. $6.00.

Here is the work of a medical historian who has groveled at the grass roots, making sure that the fabric he has wrought exhibits the unbroken threads of medical history as it evolved in the State of Texas. In evidence of the above, the author has produced two important previous volumes which serve as an historical background for this interesting story. They are *The Medical Story of Early Texas 1528-1853* leading up to the organization of the Texas State Association, and *A Century of Medicine in San Antonio*, also historically related. These works are mentioned because the required research in their preparation eminently qualified the author for the philosophical and accurate portrayal of the succeeding century of organized medicine in the same area under the impact of the forces he had so thoroughly studied and reported.

Here is a carefully organized, well written, thoroughly documented story of medical development with its multifaceted impact upon community life in the great State of Texas, a sizable section of the United States.

This book dedicated "To the Founders of the Texas Medical Association" and introduced by an apt quotation from William Osler deserves a wide reading. The

history of medicine anywhere is important to people everywhere. Dealing with the most vital issues of life, medicine is intimately connected with the progress of civilization and belongs to the world.

Many readers may be surprised to learn that the Texas Medical Association was organized only six years after the American Medical Association was founded and that the program of the two organizations has been coeval and in many respects complementary. Texas has had one A. M. A. President and has filled many national offices of lesser significance. The all important movement for voluntary health insurance, now occupying the best minds in the nation, originated in Dallas. Furthermore, it is said that the Woman's Auxiliary to the American Medical Association had its origin in the State of Texas.

History indicates that on the whole Texas physicians were rugged individualists of unimpeachable integrity contending for personal initiative and professional freedom. Organized medicine in Texas has served as a bulwark against compulsory health insurance and the trend toward general socialization.

In spite of the factual content and the necessary detail required for this book the reading is easy. The language is chaste and simple causing the story to travel on its own power. This suggests that the writing must have demanded much meticulous thought and at best it must have been difficult. Perhaps the author remembered that Richard B. Sheridan said, " Easy writing's curst hard reading."

Historically this is not only an authentic story of organized medicine in the State but biographically it presents many of the interesting and colorful pioneer personalities who brought applied medicine to the plains and made effective organization possible, their emotions, their achievements and occasionally their prejudices and their vices appear. All these make history. Nearly 100 of these exceptional characters appear in the reproductions of portraits and photographs and there are pictures of the new home of the Texas Medical Association dedicated at the recent centennial meeting. The obvious features are structural beauty and utility.

After following the varying vicissitudes of these pioneering physicians through a century of progress in peace and war and after sharing their hope for a permanent home through a half century of striving and planning, it is good to think with the author: " As one walks up the front steps and looks up at the beautiful doors, there is an inviting friendliness about the building which seems to say to the doctors of Texas, ' enter in and be refreshed and renewed in mind and spirit.' "

Harking back to the foreword to this volume we join Merton M. Minter, M. D. in saying " we can better understand the science and we can better learn the art if we know what the physicians of the past century were doing in medicine." The truth of this statement appears in the author's interesting preface as attested by these two sentences:

" As we write and as we read about these men and what they accomplished, we should rejoice with unceasing reverence and pride that they are our medical forefathers. As the result of their work and as a tribute to their attainments, we should accept our obligations and opportunities with greater assurance and greater courage."

LEWIS J. MOORMAN

PHILIP D. JORDAN, *The People's Health. A History of Public Health in Minnesota to 1948.* Saint Paul: Minnesota Historical Society, 1953. xli + 524 pp. $5.00.

The author of this study is professor of history at the University of Minnesota. He has written many interesting articles in the field of medical history. This previous preparation adds materially to his authorship of this volume, the subject of which is a natural selection for one with his extensive background.

It is stated that *The People's Health* is "an informal history written for the general reader, emphasizing the social aspects of public health and the roles of persons and organizations in the program." While this statement has over-all validity, to this reviewer the bulk of the contents of the book deal primarily with the Minnesota State Board of Health and particularly its executive officers.

There is ample evidence that much meticulous research was undertaken, including the official records of the State Board of Health, the contemporary newspapers and the personal papers (such as reports and letters), of the secretaries of the State Board of Health from Dr. Charles N. Hewitt, 1872 to the present incumbent, Dr. Albert J. Chesley.

Dr. Hewitt served as Secretary of the State Board of Health for twenty-five years. He was followed by Dr. Henry M. Bracken, who held the position for twenty-two years. Dr. Charles E. Smith was secretary for a brief two years and was succeeded by Dr. Chesley in 1921 who now is serving.

Minnesota was the fourth state in the United States to establish a State Board of Health. This was in 1872—her predecessors being Massachusetts in 1869; California in 1870; and the District of Columbia in 1871.

Dr. Hewitt of Red Wing was the father of the State Board of Health idea in Minnesota. He had been an Army Surgeon during the Civil War and must have come into contact with men from Massachusetts. In any case it is interesting to speculate upon the reasons for the wide territorial dispersal in the establishment of the early state boards of health.

To this reviewer it seems doubtful that the general reader will sustain interest in the detailed struggles each of the secretaries encountered to establish a public health program in Minnesota. The book rather should appeal to the professional worker engaged in public health. Viewed in this manner the author has made a distinct contribution in recording the history of the development of public health in Minnesota.

Thus the volume is a welcome addition to a gradually expanding list of histories of state participation in the field of public health.

ROBERT G. PATERSON

LLOYD G. STEVENSON. *Nobel Prize Winners in Medicine and Physiology 1901-1950.* New York: Henry Schuman, 1953. ix + 291 pp. Ill. $6.50.

This is the third volume in Henry Schuman's series, "A Half Century of Nobel Prize Winners in Physics, Chemistry, Medicine and Physiology." Dr. Stevenson has arranged his material in the following manner: immediately following the laureate's name and dates is a quotation giving, often in a single phrase, the reason

for his selection for the honor, e. g. in the case of Robert Koch, " For his investigations and discoveries in regard to tuberculosis." Then come first a very brief biographical sketch, next a quotation in which the laureate describes in his own words his prize-winning discovery, and finally a section entitled " Consequences in Theory and Practice " in which Dr. Stevenson explains the importance of the discovery and relates it to our present day knowledge of the field concerned Included also is a portrait of each of the recipients.

Since 59 persons are considered, the space which can be devoted to each in a book of this size is necessarily limited. The reader is conscious of this condensation and his appetite, especially for a few more biographical details, is excited rather than satisfied. The selections in which the laureate explains his discovery are excellently chosen, and technical terms are carefully translated for the lay reader. Particularly interesting are those passages in which the laureate tells how he happened upon his discovery. Dr. Stevenson's comments at the end of each account are most illuminating, and taken together give an excellent picture of the advances in medicine during the last half century, from von Behring's introduction of serum therapy for diphtheria in 1890 to the isolation and therapeutic use of certain of the suprarenal cortex hormones worked out in the 1940's by Kendall, Hench, and Reichstein.

J. M. D. Olmsted

Sir D'Arcy Power and W. R. LeFanu. *Lives of the Fellows of the Royal College of Surgeons of England: 1930-1951.* London: Published by the College, 1953. xii + 889 pp. 42 shillings.

This book is a continuation of *Plarr's Lives of the Fellows of the Royal College of Surgeons of England*, published in two volumes in 1930. The late Sir D'Arcy Power wrote the obituaries down to October, 1940; Mr. LeFanu has completed the task through 1951, and has edited the whole. It is hoped that a further continuation may be published in ten years' time.

These notices are almost certainly more accurate than those published from time to time in the periodical press. What is more, they show remarkably little obituary piety, Power having adopted as his principle, " No flowers." Partly on this account, partly because the compendious writing of both authors is crisp and clear but largely because the lives recorded are so often colorful, dull pages are few. The proportion of hardtack in the dietary—" lives " which are little more than lists of dates, with degrees, appointments, honors and the like—is quite surprisingly small. Surgery, one supposes, must attract individualists. At any rate a rather high percentage of the Fellows seem to have marked individual characteristics. Hodge and hero, worldling and near-saint are tumbled together by the chance of the alphabet. Why, by the way, should a volume so arranged be supplied with an index of names? It seems a little redundant.

To reach back from yesterday as far as the days when Listerism was new is to comprehend a fascinating chapter in the history of surgery. The survey is not so incomplete, because national, as might be supposed. The Fellows of the Royal College come from, and go to, all parts of the world, and leading surgeons of many

lands are elected Honorary F. R. C. S. In British surgery the development of the specialities—particularly the surgery of the eye, the genito-urinary tract, the brain, the thorax, the extremities, and the vascular system—is clearly indicated, side by side with the persistent tradition of general surgery. Changes in the basic-science training of the surgeon may also be discerned—for example, the rise of pathology and the lessening, though still strong, emphasis on anatomy, followed by the yet later inclination toward the physiological sciences. Strictly surgical accomplishment is also represented by Bland-Sutton, Robert Jones, Arbuthnot Lane, Mayo-Robson, Moynihan, Trotter, and many others.

Apart from an oracular certainty about "firsts"—nine in ten of such claims, here or elsewhere, are debatable—this book gives little ground for complaint. It is a job well done, and the series of which it is part should not be allowed to lapse. One imagines that elderly Fellows sometimes cast a speculative eye at their genial librarian, as he lopes through the reading room like a cheerful nemesis, wondering what sort of summation is in store for them. May it comfort them to know that their official obituaries will be accurate, objective and readable.

LLOYD G. STEVENSON

BERNHARD ASCHNER. *Lehrbuch der Konstitutionstherapie.* 7. Auflage. Technik der Allgemeinbehandlungsmethoden mit zahlreichen Beispielen aus der Praxis. Stuttgart: Hippokrates-Verlag Marquardt & Cie., 1953. 713 pp. D. M. 46. 50.

Dr. Aschner was formerly professor of gynaecology at the University of Vienna. He is well known for his Paracelsus translation into modern German and, strangely enough, still better known for the sign which carries his name. Like other physicians of his generation, he became in his mature years dissatisfied with the actual structure of academic medicine, more particularly, with the therapeutical fatalism and nihilism, siginficant of the once so powerful anatomical concept of disease in the late 19th and the early 20th centuries. Aschner developed a *flexible* conception of constitution, thereby reviving the teaching of the medical school of Cos and its belief in the shaping influences of climate, atmosphere, seasons, soil, life periods, social and even psychological factors. It was only natural that Aschner joined that group of physicians, who, in the early thirties tried to revive a less rigid and more complete idea of health and disease, trusting that a new synthesis of medical knowledge at large would prevent our art from further fragmentation. *Hippokrates* was the organ of these "Unitarian Tendencies of Contemporary Medicine" (*Zeitschrift fuer die Einheitsbestrebungen der Gegenwartsmedizin*). In these tendencies were included representatives of all branches of medicine, homeopaths and psychoanalysts, at the time far from enjoying the legitimate position they are now holding. Medical historians were in the forefront of this movement; chief editor was G. Honigmann, professor of medical history (Giessen), and the editorial staff listed among others: H. Sigerist (Leipzig), R. Koch, (at that time professor of medical history, Frankfurt am Main), P. Schilder (Vienna, later New York), H. Meng, at present professor of mental hygiene, (Basel, Switzerland), H. Much, professor of hygiene (Kiel); A. Meyer (Baltimore), and C. von Monakow (Zuerich) were among the contributors. The papers published in *Hippokrates* could not be of equal value, since

the journal was not of a traditional character, but rather a manifesto of a vanguard in contemporary medicine. Its role was intended to be a historical rather than a purely informative one, and the surviving members of the group feel that the periodical did fulfill its mission, though prematurely disrupted by the approaching cultural disaster.

It was necessary to recall and to revive this atmosphere for a better understanding and just evaluation of Aschner's book. The efforts of the author often were received by his readers with a deplorable lack of realism and historical sense. This reviewer's task has been limited to the historical part of the book which comprises 190 out of 700 pages. But the special chapters on constitution, universal methods of treatment (*Allgemeinbehandlungsmethoden*), and local diseases (*Klassifikation der Krankheiten*) also contain historical notes in abundance. Aschner is above all a physician and his approach toward the history of medicine primarily pragmatic, that is, his main, if not exclusive interest is therapy and cure: "Any philosophy of medicine not resting on *healing* as its *foundation* is useless for the practitioner" (p. 158). His outline of medical history is chiefly that of a history of treatments. Since he is convinced that "human nature as well as disease have undergone but little change since antiquity" (p. 92), nothing indeed stood in the way of his life long endeavor to unearth the therapeutical treasure of the past, his final intention being to save and to use it for present humanity. His work is unique, a one-man performance not equalled by any of the few attempts of a similar kind. The reader anxious to learn the therapeutical procedures used from prehistoric to modern times will not be disappointed. Whatever may be the truth of Aschner's belief in an undiminished usefulness of therapeutical methods, designed for past generations and civilizations, we have to be grateful for the collection and organized presentation of an almost overwhelming material, the rebirth of a forgotten and neglected art, and the rediscovery of old remedies and methods which could not have been useless in the hands of eminent physicians who applied them to their own satisfaction. The book also strongly testifies to the all too frequently neglected significance of historical studies for the mastery of the actual tasks the physician has to face.

Aschner, though man of *action*, could not avoid facing medical *thought*. He repeatedly defines his stand by a number of recurrent terms, intended to express his opposition to "laboratory medicine," "experimental medicine," "mechanistic medicine," "localized treatment." He objects to the exaggeration of rationalistic and causal thought in medicine, to the exclusively inductive and analytic method, and to the antihistorical mode of reasoning. From these and related connotations one can easily derive the antithetic terms, which, according to Aschner, should be given full credit in future developments. There is no doubt that much truth is embodied in his polemic, though this section could have gained by a greater precision in concepts and a more sober and discriminative use of those terms which were challenged by Aschner. The underlying epistemological theme of the entire work is the attempted reconciliation of empirical and rational medicine, a reconciliation he hopes to achieve precisely through his constitutional therapy, taking into consideration man as a whole, his complexion, sex, age, temperament and predominant organ, to cite but a few of the physical constituents of human nature. The universality of the constitutional approach and the design of a therapy derived from it, marks the Hippocratic spirit of the author and his work.

WALTHER RIESE

P. DIEPGEN. *Das Analzäpfchen in der Geschichte der Therapie.* Stuttgart: Georg Thieme Verlag, 1953. 44 pp. Ill. D. M. 4.20.

This booklet traces the vicissitudes of the therapeutic use of anal suppositories from ancient Mesopotamia to the twentieth century. Vicissitudes, rather than development, seems the correct expression, since favor and disfavor in which this form of treatment were held give its history a very irregular appearance. Although the compilation of the material is not exhaustive, it is representative enough to illustrate the various preparations as well as indications during the periods under discussion. Moreover, the author's meticulous technique of using and quoting original sources provides a secure basis and his clear style avoids misunderstanding.

Although the topic is not of prime importance, it is yet interesting, especially because of Professor Diepgen's ability to relate it to larger issues in the history of medicine. To give but an example: the ability of the body to absorb substances from the rectum was hardly doubted as long as blood vessels, lymphatic ducts, and hollow nerves were believed to end freely in the intestinal canal. It was the advent of the cellular theory which made this assumption questionable and compelled a new experimental approach (p. 33 ff.). Examples like this bring out the bearing of general theories upon matters of detail in sharp relief, and help the reader to understand the ramifications of medical thought and practice.

OWSEI TEMKIN

THE BULLETIN RECENTLY RECEIVED *

Robert J. Benford. *Doctors in the Sky.* The Story of the Aero Medical Association. Springfield, Ill.: Charles C Thomas, 1955. xv+326 pp. Ill. $8.75.

Richard M. Burke. *An Historical Chronology of Tuberculosis.* 2nd edition. Springfield, Ill.: Charles C Thomas, 1955. xiii+125 pp. Ill. $3.75.

A. C. Crombie. *Oxford's Contribution to the Origins of Modern Science.* Oxford: Basil Blackwell, 1954. 27 pp. 2s. 6d.

Reuben Friedman. *A History of Dermatology in Philadelphia.* Including a Biography of Louis A. Duhring, M. D., Father of Dermatology in Philadelphia. Fort Pierce Beach, Fla.: Froben Press, 1955. 556 pp. Ill. $10.00.

Merrill Moore. *Verse-Diary of a Psychiatrist.* Volume 7, Contemporary Poetry Library Services, Ed. Mary Owings Miller. Baltimore: Contemporary Poetry, 1954. 39 pp. $3.00.

The President's Review. Reprinted from The Rockefeller Foundation Annual Report, 1953. New York: Rockefeller Foundation, 1954. 61 pp.

W. Riese. *La Pensée Morale en Médecine.* Paris: Presses Universitaires de France, 1954. xi+81 pp. 348 Fr. francs.

* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section.

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